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ANOTHER blunder has been committed by the Post-office clerks, who charged postage, as for a letter, on the wrapper of vol. 32, *because it was on blue paper!* This mistake was immediately corrected by the higher authorities, who directed the extra postage to be returned, on exhibition of the cross bands. Dr. Petersen has now caused the title to be printed on the cover, so that it is hoped all will go on smoothly for the future. If the carelessness or ignorance of the subordinates at the Post-office cause any further hindrance, application will be made for the removal of the offending party. It is not to be endured that the liberal intentions of the Government should be thwarted by such agents.

Recently published, price 24*s*., *General-Register der Bände* I. bis XX., *der Astronomischen Nachrichten*, ausgearbeitet von G. A. Jahn, Phil. Doct., &c.

In a letter dated Nov. 3, Mr. Lassell announces the discovery of two interior satellites of *Uranus*.

Discovery of a new Planet, by Dr. A. de Gasparis.

This planet, which is estimated by Dr. de Gasparis as of a fair 9th magnitude, was discovered by him on July 29. It has received the name of *Eunomia*.

EUNOMIA.

NAPLES. (Dr. A. de Gasparis.)

1851.	Naples M.T. h m s	App. R.A. h m s	App. Dec. ° ′ ″
July 29	11 44 53·7	18 15 59·94	−26 3 54·0
30	10 17 59·9	15 22·17	25 59 49·1
31	10 52 57·0	14 41·26	55 1·8
Aug. 2	11 17 37·9	13 26·89	46 26·1
3	9 31 4·2	12 53·98	41 57·4
4	12 9 18·0	12 18·25	37 3·9
5	11 54 22·6	11 47·17	32 31·2
8	11 40 32·5	10 25·09	19 30·9
9	10 11 23·3	10 2·93	15 13·9
10	10 2 50·1	9 41·03	10 46·0
11	9 11 24·7	9 21·72	6 31·1
12	10 20 28·5	18 9 1·79	−25 2 10·8

PADOVA. (M. Trattenero.)

1851.	Padova M.T. h m s	App. R.A. h m s	App. Dec. ° ′ ″	
Aug. 13	9 46 55·8	18 8 46·90	−24 58 19·2	4 Comparisons.
14	10 7 19·5	8 31·77	53 41·9	3 —
15	8 33 50·6	8 18·53	49 37·7	Meridian.
16	29 43·7	8 7·36	45 20·6	—
17	25 37·4	7 57·11	41 5·5	—
20	13 32·6	7 39·91	28 27·3	—
21	9 34·6	7 37·83	24 17·9	—
22	5 37·9	7 36·96	20 2·0	—
23	8 1 42·9	7 37·86	16 6·1	—
24	7 57 50·5	18 7 41·42	−24 12 3·3	—

HAMBURG. (M. C. Rümker.)

1851.	Hamburg M.T. h m s	App. R.A. ° ′ ″	App. Decl. ° ′ ″
Aug. 14	9 30 48·0	272 7 57·4	−24 52 47·3
19	9 10 30·6	271 55 50·8	24 32 46·5
28	8 25 43·9	272 2 57·0	23 56 3·5
29	8 37 24·7	5 59·0	51 55·7
30	9 5 32·2	9 22·9	47 57·9
31	8 29 41·5	13 13·5	44 14·3
Sept. 6	8 7 17·8	44 49·3	21 42·0
8	8 25 26·3	272 58 41·5	14 17·2
9	8 2 2·0	273 6 0·0	23 10 50·2
18	8 23 3·9	274 30 8·7	22 39 7·0
22	7 35 6·9	275 15 51·6	25 22·3
24	7 50 16·2	41 25·8	18 12·6
25	7 18 3·4	275 53 53·4	15 16·2
27	7 50 3·1	276 21 21·9	−22 8 16·1

The first two observations are corrected for parallax, those which follow are not corrected.

Mean Places for Jan. 0, 1851, of Stars in the path of *Eunomia*, by M. C. Rümker.

	Mag.	R.A. h m s	Decl. ° ′ ″		Mag.	R.A. h m s	Decl. ° ′ ″
(1)	9	18 0 31·73	−24 44 7·2	(7)	9	18 5 26·10	−24 36 6·8
(2)	9	2 32·86	28 39·8	(8)	9	5 27·50	45 6·5
(3)	9	3 37·29	40 18·0	(9)	10	5 51·06	32 36·1
(4)	9	3 39·06	30 55·1	(10)	10	6 51·22	34 42·0
(5)	8·9	4 25·01	37 34·4	(11)	6	18 12 21·07	−24 58 34·8
(6)	11	18 4 45·43	−24 42 5·0				

(1), (5), (7) have been determined by the meridian circle, the others by the equatoreal; (10) is rather doubtful.

ROME. (Professor Secchi.)

1851.	Rome M.T. h m s	App. R.A. h m s	App. Dec. ° ′ ″	
Aug. 6	9 12 16·5	18 11 21·34	−25 28 58·4	Merid. Circle.
7	7 53·4	10 54·03	24 32·1	— —
8	9 3 31·9	10 28·38	20 4·5	— —
9	8 59 12·4	10 4·70	15 39·7	— —
10	54 54·2	9 42·37	11 14·2	— —
11	50 37·9	9 21·94	6 56·1::	— —
12	8 46 23·4	18 9 3·23	−25 2 53·1	— —

CAMBRIDGE. Mer. Circle and Northumberland Equat. (Prof. Challis.)

1851.	Greenwich M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	No. of Comps. in R.A.	N.P.D.	Star.
Aug. 15	9 51 51·7	18 8 17·68	+8·182	114 49 10·0	−9·9808	3	3	*a*
19	8 17 1·8	7 44·50		32 28·2	9·9871	Meridian.		
	9 36 39·2	43·63	8·185	32 10·0	9·9800	8	3	*a*
20	8 13 1·0	7 39·58				Meridian.		
21	8 9 3·3			24 4·5	9·9868	Meridian.		
	10 8 37·3	7 37·65	8·351	23 38·1	9·9713	6	6	*a*
22	8 5 6·9	7 37·26		20 0·0	9·9867	Meridian.		
	9 18 5·5	37·23	8·149	19 42·7	9·9809	2	2	*b* & *c* equally
23	9 43 48·1	7 38·61	8·290	15 36·5	9·9751	3	3	*b*
	9 43 51·3	38·69	8·290	114 15 37·1	9·9751	2	2	*c*
28	9 48 19·4	8 12·44	8·370	113 55 25·7	9·9690	4	4	*d*
Sept. 3	10 25 40·3	9 51·67	8·510	32 9·8	9·9494	2	2	*d*
9	8 52 17·2	18 12 26·03	+8·325	113 10 38·8	−9·9712	10	6	*e*

Assumed *Mean* Places of the Stars, 1851·0.

	R.A. h m s	N.P.D. ° ′ ″	Name of Star.
a	18 12 21·01	114 58 33·9	B.A.C. 6217
b	17 53 43·34	114 16 33·4	7 Sagittarii = B.A.C. 6097
c	17 56 2·36	114 24 2·2	B.A.C. 6111
d	18 7 28·14	113 56 31·9	H.C. 33516
e	18 13 1·11	112 59 3·6	B.A.C. 6222

The places of the stars in the respective catalogues have been adopted, with the exception of those of B.A.C. 6217 and B.A.C. 6222, which were not to be depended on as being deduced from Lacaille alone. The assumed place of 6217 was determined by four transit and four circle observations taken at the Cambridge Observatory. The N.P.D. of 6222 was obtained by two circle observations, and its R.A. by two equatoreal comparisons with each of the stars B.A.C. 6286 and B.A.C. 6343. The seconds of the R.A. of B.A.C. 6286 in the catalogue should be 22·57 instead of 20·46, a wrong value of the annual variation having been used to derive it from the Cambridge Observations of 1834.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1851.	Green. M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$	No. of Comps. in R.A.	N.P.D.	Set.
	h m s	h m s		° ′ ″				
Sept. 12	8 9 0·0	18 14 4·27	+8·172	112 59 54·6	−9·9820	4	4	1
13	8 12 33·8	14 40·23	8·158	56 21·9	9·9824	24	8	2
15	8 25 0·1	15 57·82	8·245	49 31·2	9·9791	24	8	3
21	8 20 27·4	18 20 18·30	+8·301	112 28 33·8	−9·9757	21	7	4

P is the equatoreal horizontal parallax in seconds of arc; p and q are the required corrections in time and arc respectively.

Mean Places of Stars of Comparison, 1851·0.

Name.	R.A.	N.P.D.	Set.
	h m s	° ′ ″	
Lalande, H.C.33885	18 16 6·25	113 3 14·3	1, 2
B.A.C. 6222	13 1·92	112 59 14·9	3
Lalande, H.C.34222	18 23 31·37	112 23 38·5	4

Bessel's precessions for the mean year have been applied to Lalande's stars, and not those given in the reduction by Baily.

Elements.

I. Professor Encke.*

M	172 10 21·6	1851, Aug. 5, 0h Berlin M.T.
π	112 18 15·6	
Ω	292 51 1·8	
i	13 0 18·5	
φ	7 50 44·2	
Log a	0·380110	
μ	954″·645	

II. MM. Westphal and Klinkerfues, of Göttingen, from the Naples Observations of July 29, August 5, and August 12.

M	170 40 26·5	1851, Aug. 5, 0h Berlin M.T.
π	113 25 14·5	True Equinox, August 5.
Ω	292 48 47·9	
i	13 3 5·8	
Log a	0·3803271	
Log e	9·1391746	
Log μ	2·9795160	

* These elements are calculated on the Naples Observations to August 12, which they represent very fairly: the Professor also calculated an approximate ephemeris for the immediate use of observers, which is omitted.

III. Signore Trattenero, of Padova, from the Naples Observations of July 29, Aug. 10, and the Padova Observation of Aug. 21.

M	240° 36′ 39″·9	1851, July 30, 0^h Green. M.T.
π	56 23 14·4	Mean Equinox, July 30.
Ω	293 42 6·3	
i	12 1 34·2	
φ	7 13 54·3	
Log a	0·393714	
μ	910″·82	

IV. By Mr. G. Rümker, from the Naples Observation of July 29 and the Hamburg Observations of Aug. 29 and Sept. 27.

M	293° 49′ 51″·49	Oct. 1, 0^h G. M. T. 1851.
π	27 35 37·57	M. Eq. Jan. 0, 1851.
Ω	293 52 54·76	
i	11 43 42·75	
φ	10 51 34·29	
e	0·1884017	
Log a	0·4228496	
Log μ	2·9157322	

These elements represent the middle observations, thus:—

Computation − Observation = $\begin{cases} -0''\cdot07 \text{ in Longitude.} \\ 0''\cdot00 \text{ in Latitude.} \end{cases}$

Ephemeris.

Computed from the above Elements by M. G. Rümker.

For $9^d\ 36^m$ Greenwich M. Time.

1851.	R.A. h m s	Decl. ° ′	Log Δ.	1851.	R.A. h m s	Decl. ° ′	Log Δ.
Oct. 1	18 29 22	−21 53·9	0·37080	Oct. 16	18 46 21	−20 59·9	
2	30 23	50·5		17	47 36	56·1	0·40207
3	31 25	47·0		18	48 53	52·3	
4	32 28	43·5		19	50 10	48·3	
5	33 32	40·0	0·37893	20	51 28	44·4	
6	34 37	36·5		21	52 47	40·4	0·40935
7	35 43	32·9		22	54 6	36·4	
8	36 50	29·4		23	55 27	32·2	
9	37 57	25·8	0·38684	24	56 49	28·1	
10	39 6	22·2		25	58 11	24·0	0·41637
11	40 16	18·5		26	18 59 34	19·8	
12	41 27	14·9		27	19 0 58	15·6	
13	42 39	11·1	0·39456	28	2 22	11·3	
14	43 52	7·4		29	3 47	7·0	0·42314
Oct. 15	18 45 6	−21 3·6		Oct. 30	19 5 13	−20 2·5	

1851.	R.A. h m s	Decl. ° ′	Log Δ.
Oct. 31	19 6 40	−19 58·1	
Nov. 1	8 7	53·6	
2	9 34	49·1	0·42968
3	11 3	44·5	
4	12 32	39·8	
5	14 2	35·1	
6	15 32	30·3	0·43595
7	17 3	25·5	
8	18 35	20·5	
9	20 7	15·5	
10	21 40	10·5	0·44197
11	23 14	5·3	
12	24 48	−19 0·1	
13	26 22	−18 54·9	
14	27 57	49·6	0·44771
15	29 32	44·2	
16	31 7	38·7	
17	32 44	33·2	
18	34 21	27·5	0·45320
Nov. 19	19 35 58	−18 21·8	
Nov. 20	19 37 35	−18 16·1	
21	39 13	10·2	
22	40 53	−18 4·3	0·45844
23	42 32	−17 58·3	
24	44 12	52·2	
25	45 52	46·2	
26	47 32	40·0	0·46340
27	49 13	33·8	
28	50 54	27·5	
29	52 35	21·0	
30	54 17	14·5	0·46811
Dec. 1	56 0	7·9	
2	57 43	−17 1·2	
3	19 59 26	−16 54·4	
4	20 1 9	47·5	0·47256
5	2 52	40·6	
6	4 36	33·6	
7	6 21	26·5	
8	20 8 6	−16 19·4	0·47675

Earlier Elements and Ephemerides by M. G. Rümker are omitted.

IRENE.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1851.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	Star of Comp.
Aug. 1	9 57 7·6	15 40 41·53	+8·439	108 15 22·2	−9·9543	B.A.C. 5176
	10 13 7·5	41·71	+8·472	25·7	−9·9486	— —

The following is the assumed *mean* place of the star of comparison for 1851·0, derived from the Greenwich 12-year Catalogue:—

	R.A. h m s	N.P.D. ° ′ ″
B.A.C. 5176	15 33 22·20	109 11 30·54

HAMBURG. (M. Rümker.)

1851.	Hamburg M.T. h m s	App. R.A. ° ′ ″	App. Dec. ° ′ ″
July 22	10 41 5·0	233 53 20·0	−17 18 43·2
24	10 30 29·0	234 5 47·6	−17 29 44·5

Not corrected for parallax.

Mean Places for January 0·1851 of the two Stars with which the Planet was compared both days.

R.A. h m s	Dec. ° ′ ″
15 38 49·37	−17 37 17·3
15 41 20·75	−17 26 32·4

CAMBRIDGE. Northumberland Equatoreal. (Prof. Challis.)

1851.	Greenwich M.T.	App. R.A.	Log $\frac{p}{P}$	App. N.P.D.	Log $\frac{q}{P}$	No. of Comps. in R.A.	No. of Comps. in N.P.D.	Star.
	h m s	h m s		° ′ ″				
May 21	11 0 5·2	16 2 15·06	−8·076			5		*a*
	12 17 58·3	11·92	+7·353	103 26 28·6	−9·9579	9	9	*a*
July 2	10 39 0·7	15 33 11·74	8·284	105 36 28·3	9·9566	6	6	*b*
4	10 55 49·4	32 55·07	8·367	105 45 54·2	9·9527	8	8	*b*
10	11 10 7·1	32 47·97	8·461	106 15 3·3	9·9452	2	2	*c*
12	11 3 59·5	32 59·06	8·465	25 18·8	9·9452	5	5	*c*
16	10 26 38·0	33 41·89	8·415	45 53·5	9·9512	1	1	*d*
	10 38 18·1	41·65	8·443	65·9	9·9485	6	6	*c*
21	10 19 9·9	35 10·79	8·442	107 13 6·9	9·9496	3	2	*e*
29	10 0 38·0	38 53·24	8·463	107 57 58·2	9·9484	2	2	*f*
Aug. 13	9 1 52·1	49 47·09	8·444	109 25 27·5	9·9536	3	3	*g*
	9 21 3·2	47·43	8·484	33·3	9·9481	3	3	*h*
21	9 3 25·1	57 25·37	8·498	110 12 24·0	9·9469	9	9	*i*
23	8 52 57·7	15 59 30·00	8·490	110 24 2·4	9·9487	6	6	*k*
Sept. 10	7 58 40·8	16 21 1·96	8·485	112 4 25·5	9·9521	3	1	*l*
11	7 49 14·3	22 20·86	8·471	9 40·7	9·9545	2	1	*l*
	7 56 11·7	21·67	8·485	50·4	9·9522	2	1	*m*
13	7 30 11·7	25 1·57	8·441	20 11·0	9·9591	1	1	*l*
	7 51 53·1	3·01	8·488	19·1	9·9521	9	5	*m*
15	7 41 39·1	27 47·12	8·478	30 33·9	9·9540	9	5	*m*
17	7 32 19·7	30 33·64	8·470	40 53·8	9·9555	10	5	*m*
19	7 18 35·7	33 23·46	8·451	50 36·4	9·9585	4	2	*m*
	7 31 18·9	24·82	8·479	39·2	9·9544	6	4	*n*
20	7 24 16·7	16 34 50·31	+8·470	112 55 34·9	−9·9559	8	5	*n*

Assumed *Mean* Places of the Stars, 1851·0.

	Mean R.A.	Mean N.P.D.	Name of Star.
	h m s	° ′ ″	
a	16 2 43·20	103 35 54·2	Bessel (Weisse) xvi. 38
b	15 34 23·62	105 31 57·5	B.A.C. 5184
c	35 30·12	106 23 32·3	H.C. 28617
d	28 10·80	106 30 45·9	— 28389
e	30 13·50	107 10 12·2	— 28453
f	41 31·41	107 40 49·0	— 28786
g	54 28·20	109 25 12·7	— 29156,8
h	44 41·60	109 43 1·9	λ Libræ = B.A.C. 5251
i	58 6·01	110 15 37·4	ω^1 Scorpii = B.A.C. 5337
k	58 40·45	110 27 39·0	ω^2 Scorpii = B.A.C. 5342
l	16 15 32·46	112 17 57·8	H.C. 29826
m	29 24·35	112 35 3·7	— 30207
n	16 32 15·95	113 1 45·0	— 30295

"These mean places have been derived from the respective catalogues named above. The right ascension of H.C. 29826 having been found to be about 3^{s} in error, the assumed value has been deduced from the *Histoire Céleste* on the supposition that the star was observed at the *third* wire."

CAMBRIDGE. On the Meridian. (Prof. Challis.)

1851.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Log $\frac{q}{P}$
May 21	12 5 41·8	16 2 11·99	103 26 28·5	−9·9580
22	12 0 46·3	16 1 12·18	28 1·8	9·9581
26	11 41 4·0	15 57 12·90	35 14·2	9·9585
28	31 14·9	55 15·31	39 14·5	9·9587
31	16 35·1	52 22·77	45 38·8	9·9591
June 2	11 6 52·1	50 31·22	103 50 24·3	9·9594
4	10 57 12·9	48 43·54	.....	.....
16	10 0 56·5	39 36·64	104 31 30·1	9·9617
17	9 56 24·1	39 0·04	34 59·2	9·9619
19	47 24·9	37 52·50	42 13·7	9·9623
21	9 38 31·4	15 36 50·61	104 49 45·9	−9·9627

Elements.

Computed by Mr. G. Rümker, from the observation made at London May 19, a mean of the Berlin and Cambridge observations July 21, and the Cambridge observation Sept. 20.

	° ′ ″	
M	74 35 20·56	1851, Sept. 20·0, Berlin M.T.
π	178 42 34·87	Mean Equinox,
Ω	86 50 23·98	Jan. 0, 1852.
i	9 6 14·49	
φ	9 43 37·83	
Log e	9·2277759	
Log a	0·4123316	
Log μ	2·9315092	

These elements represent the middle observation, thus :—

Calculation − Observation −0″·04 in longitude.

— — +0″·02 in latitude.

Ephemeris.

By MM. G. Rümker and O. Frömmeling.

For 9^h 36^m Berlin Mean Time.

1851.	R.A. h m s	Decl. ° ′	Log Δ.	1851.	R.A. h m s	Decl. ° ′	Log Δ.
Sept. 1	16 9 44	−21 15·7		Sept. 10	16 21 8	−22 5·1	
2	10 57	21·3		11	22 28	10·4	0·40015
3	12 11	26·9	0·37794	12	23 49	15·7	
4	13 25	32·5		13	25 11	20·9	
5	14 40	38·0		14	26 33	26·0	
6	15 56	43·5		15	27 56	31·1	0·40905
7	17 13	48·9	0·39018	16	29 19	36·2	
8	18 31	54·3		17	30 43	41·2	
Sept. 9	16 19 49	−21 59·7		Sept. 18	16 32 8	−22 46·2	

1851.	R.A. h m s	Decl. ° ′	Log Δ.
Sept. 19	16 33 34	−22 51·2	0·41927
20	35 0	−22 56·1	
21	36 27	−23 0·9	
22	37 54	5·7	
23	39 22	10·5	0·42841
24	40 51	15·2	
25	42 21	19·8	
26	43 51	24·4	
27	45 22	29·0	0·43726
28	46 54	33·5	
29	48 26	37·9	
30	49 58	44·2	
Oct. 1	51 31	46·5	0·44584
2	53 4	50·8	
3	54 38	55·0	
4	56 13	−23 59·1	
5	57 48	−24 3·2	0·45414
6	16 59 24	7·2	
7	17 1 0	11·1	
8	2 37	14·9	
9	4 14	18·7	0·46214
10	5 52	22·4	
Oct. 11	17 7 30	−24 26·1	
12	9 9	−24 29·7	
13	10 48	33·3	0·46985
14	12 28	36·7	
15	14 8	40·1	
16	15 48	43·4	
17	17 29	46·7	0·47727
18	19 10	49·9	
19	20 52	53·0	
20	22 35	56·1	
21	24 17	−24 59·0	0·48441
22	26 0	−25 1·8	
23	27 43	4·6	
24	29 26	7·4	
25	31 10	10·0	0·49127
26	32 55	12·5	
27	34 40	15·0	
28	36 25	17·4	
29	38 10	19·7	0·49785
30	39 56	21·9	
31	41 42	24·0	
Nov. 1	43 28	26·1	
2	17 45 15	−25 28·2	0·50417

PARTHENOPE.

CAMBRIDGE. Northumberland Equatoreal. (Prof. Challis.)

1851.	Greenwich M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	No. of Comps. in R.A.	No. of Comps. in N.P.D.	Star.
July 21	13 26 5·6	1 50 48·87	−8·579	83 13 5·2	−9·8765	3	3	*a*
29	12 29 25·0	1 59 15·56	8·601	82 47 9·1	9·8812	4	4	*b*
Aug. 7	12 4 38·6	2 7 23·97	8·600	27 47·7	9·8801	8	8	*c*
19	11 30 52·4	15 35·33	8·597	20 22·4	9·8787	7	4	*d*
23	11 26 48·5	17 32·71	8·586	22 35·6	9·8833	6	2	*d*
30	11 36 5·9	2 19 54·38	−8·563	82 32 14·4	−9·8710	8	6	*e*

Assumed *Mean* Places of the Stars, 1851·0.

	R.A. h m s	N.P.D. ° ′ ″	Name of Star. h
a	1 45 48·99	83 6 5·4	Bessel (Weisse) i, 823
b	1 58 20·67	82 27 59·0	— — i, 1040
c	2 3 29·54	82 7 47·4	64 Ceti = B.A.C. 672
d	2 13 16·56	82 17 49·6	Bessel (Weisse) ii, 206
e	2 21 4·97	82 17 25·4	— — ii, 347

These places were derived from the respective catalogues.

HAMBURG. (M. Rümker.)

1851.	Hamburg M.T. h m s	App. R.A. ° ′ ″	App. Dec. ° ′ ″	
July 24	12 30 22·7	28 30 43·2	+6 57 45·2	
27	14 31 31·7	29 19 24·5	+7 7 16·1	
30	12 31 42·1	30 3 22·0	15 20·8	
Aug. 2	12 52 9·7	30 45 56·4	22 51·8	Correct.
4	12 30 26·7	31 12 45·5	27 1·1	
5	12 37 57·0	31 25 52·2	+7 28 57·9	

Not corrected for parallax.

Elements, No. IV.

Calculated by M. R. Luther.

Epoch 1851, October 22 0^h, Mean Time, Berlin.

	° ′ ″	
M	60 46 39·76	
π	317 4 47·96	Mean Eq. 1851, Jan. 0.
Ω	124 58 56·87	
i	4 36 53·48	
φ	5 37 4·32	
μ	926″·22181	

Ephemeris.

By MM. R. Luther and E. Vogel, of Berlin.

For 0^h Berlin M.T.

1851.	R.A. h m s	Decl. ° ′ ″	Log Δ.	Time of Aberr. in parts of a Day.
Aug. 24	2 17 48·68	+7 37 28·0	0·221828	0·00951
25	18 13·37	36 28·3	·219301	946
26	18 36·41	35 19·2	·216781	940
27	18 57·79	34 0·9	·214270	935
28	19 17·49	32 33·3	·211768	929
29	19 35·49	30 56·7	·209277	924
30	19 51·80	29 11·0	·206798	919
31	20 6·39	27 16·2	·204333	914
Sept. 1	20 19·24	25 12·4	·201883	909
2	20 30·32	22 59·5	·199449	903
3	20 39·64	20 37·6	·197033	898
4	20 47·21	18 7·0	·194635	893
5	20 53·01	15 27·6	·192256	889
6	20 57·02	12 39·5	·189900	884
7	20 59·24	9 42·7	·187566	879
8	20 59·67	6 37·4	·185256	874
Sept. 9	2 20 58·29	+7 3 23·6	0·182973	0·00870

1851.		R.A. h m s	Decl. ° ′ ″	Log Δ.	Time of Aberr. in parts of a Day.
Sept.	10	2 20 55·09	+7 0 1·2	0·180717	0·00865
	11	20 50·07	6 56 30·6	·178491	861
	12	20 43·23	52 51·7	·176295	857
	13	20 34·56	49 4·7	·174132	852
	14	20 24·07	45 9·8	·172002	848
	15	20 11·77	41 7·1	·169908	844
	16	19 57·64	36 56·7	·167851	840
	17	19 41·69	32 38·8	·165834	836
	18	19 23·94	28 13·4	·163858	832
	19	19 4·38	23 41·0	·161926	829
	20	18 43·03	19 1·5	·160038	825
	21	18 19·91	14 15·4	·158199	822
	22	17 55·03	9 22·6	·156410	818
	23	17 28·41	6 4 23·6	·154672	815
	24	17 0·10	5 59 18·7	·152986	812
	25	16 30·11	54 8·0	·151356	809
	26	15 58·49	48 51·9	·149784	806
	27	15 25·25	43 30·7	·148270	803
	28	14 50·45	38 4·7	·146816	800
	29	14 14·10	32 34·5	·145425	798
	30	13 36·27	27 0·2	·144099	795
Oct.	1	12 56·99	21 22·4	·142839	793
	2	12 16·30	15 41·3	·141646	791
	3	11 34·28	9 57·3	·140523	789
	4	10 50·97	5 4 10·7	·139472	787
	5	10 6·41	4 58 22·2	·138493	785
	6	9 20·67	52 31·9	·137588	783
	7	8 33·81	46 40·6	·136757	782
	8	7 45·89	40 48·4	·136002	781
	9	6 56·95	34 55·7	·135325	779
	10	6 7·07	29 3·2	·134725	778
	11	5 16·31	23 11·1	·134205	777
	12	4 24·71	17 20·0	·133767	777
	13	3 32·35	11 30·3	·133410	776
	14	2 39·31	4 5 42·3	·133136	776
	15	1 45·65	3 59 56·6	·132944	775
	16	2 0 51·45	54 13·5	·132836	775
	17	1 59 56·78	48 33·6	·132813	775
	18	59 1·73	42 57·4	·132875	775
	19	58 6·36	37 25·3	·133021	775
	20	57 10·76	31 57·9	·133253	776
	21	56 15·01	26 35·4	·133570	776
Oct.	22	1 55 19·19	+3 21 18·5	0·133973	0·00777

1851.		R.A. h m s	Decl. ° ′ ″	Log Δ.	Time of Aberr. in parts of a Day.
Oct.	23	1 54 23·35	+3 16 7·5	0·134461	0·00778
	24	53 27·59	11 2·7	·135036	779
	25	52 32·00	6 4·9	·135696	780
	26	51 36·65	3 1 14·2	·136440	781
	27	50 41·61	2 56 31·3	·137268	783
	28	49 46·99	51 56·5	·138179	785
	29	48 52·85	47 30·1	·139171	786
	30	47 59·27	43 12·6	·140244	788
	31	47 6·33	39 4·2	·141397	790
Nov.	1	46 14·08	35 5·3	·142629	793
	2	45 22·60	31 15·9	·143938	795
	3	44 31·94	27 36·7	·145322	798
	4	43 42·17	24 7·8	·146781	800
	5	42 53·35	20 49·3	·148312	803
	6	42 5·54	17 41·7	·149915	806
	7	41 18·79	14 45·1	·151588	809
	8	40 33·15	11 59·5	·153328	812
	9	39 48·68	9 25·2	·155134	816
	10	39 5·42	7 2·3	·157005	819
	11	38 23·42	4 51·0	·158938	823
	12	37 42·72	2 51·4	·160934	827
	13	37 3·36	2 1 3·3	·162989	831
	14	36 25·38	1 59 27·2	·165102	835
	15	35 48·81	58 2·9	·167270	839
	16	35 13·69	56 50·7	·169493	843
	17	34 40·05	55 50·5	·171768	848
	18	34 7·93	55 2·3	·174093	852
	19	33 37·35	54 26·2	·176467	857
	20	33 8·35	54 2·2	·178887	862
	21	32 40·95	53 50·1	·181352	867
	22	32 15·16	53 50·2	·183859	872
	23	31 51·02	54 2·4	·186407	877
	24	31 28·53	54 26·6	·188994	882
	25	31 7·73	55 2·8	·191617	887
	26	30 48·60	55 50·9	·194275	893
	27	30 31·18	56 50·8	·196966	898
	28	30 15·45	58 2·2	·199688	904
	29	30 1·45	1 59 25·2	·202439	910
	30	29 49·15	2 0 59·5	·205218	916
Dec.	1	29 38·57	2 45·0	·208023	922
	2	29 29·70	4 41·9	·210851	928
	3	29 22·51	6 49·8	·213701	934
Dec.	4	1 29 17·03	+2 9 8·6	0·216571	0·00940

1851.	R.A. h m s	Decl. ° ′ ″	Log Δ.	Time of Aberr. in parts of a Day.
Dec. 5	1 29 13·23	+2 11 37·9	0·219461	0·00946
6	29 11·12	14 17·8	·222368	952
7	29 10·68	17 8·0	·225290	959
8	29 11·90	20 8·3	·228227	965
9	29 14·76	23 18·7	·231177	972
10	29 19·27	26 38·8	·234138	979
11	29 25·40	30 8·8	·237110	985
12	29 33·15	33 48·2	·240091	992
13	29 42·51	37 36·8	·243081	0·00999
14	29 53·45	41 34·7	·246079	0·01006
15	30 5·98	45 41·4	·249082	1013
16	30 20·08	49 57·0	·252090	1020
17	30 35·73	54 21·1	·255102	1027
18	30 52·93	2 58 53·8	·258117	1034
19	31 11·63	3 3 34·7	·261133	1041
20	31 31·86	8 23·8	·264150	1049
21	31 53·57	13 20·8	·267167	1056
22	32 16·76	18 25·7	·270183	1063
23	32 41·41	23 38·2	·273196	1071
24	33 7·50	28 58·3	·276206	1078
25	33 35·02	34 25·6	·279212	1086
26	34 3·95	40 0·0	·282213	1093
27	34 34·27	45 41·2	·285207	1101
28	35 5·96	51 29·2	·288195	1108
29	35 38·99	3 57 23·8	·291175	1116
30	36 13·37	4 3 24·9	·294148	1124
31	36 49·05	9 32·1	·297114	1131
32	37 26·01	15 45·4	·300070	1139
33	38 4·25	22 4·5	·303014	1147
34	1 38 43·72	+4 28 29·3	0·305948	0·01155

HYGEIA.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1851.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Compd—Obsd. R.A. s	Compd—Obsd. N.P.D. ″	Stars of Comp. B.A.C.
Sept. 14	11 34 36·9	0 20 3·39	82 12 11·2	−1·57	+12·3	26, 8331
	12 16 29·9	2·28	18·9	1·65	11·1	— —
15	10 16 11·9	0 19 23·97	82 15 45·7	1·50	10·7	— —
	10 56 5·1	22·83	50·6	−1·51	+12·1	— —

The assumed *mean* places of the stars of comparison are those given for *Iris*. The observed places of the planet have been corrected for refraction and parallax. The computed places were deduced from the ephemeris contained in the supplement to the *Nautical Almanac* for 1854.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1851.	Green. M.T.	R.A.	Exc. of Ephem.	N.P.D.	Exc. of Ephem.	No. of Comps. in R.A.	N.P.D.	Set
	h m s	h m s	s	° ′ ″	s			
Sept. 13	12 38 16·8	0 20 42·75	−1·59	82 8 44·5	+ 8·9	24	8	1
15	12 19 27·4	19 20·32	1·43	16 9·0	7·0	12	4	2
21	13 16 10·4	15 1·16	1·42	82 40 32·8	10·2	24	8	3
27	13 3 23·2	10 37·20	1·02	83 7 12·7	7·8	18	8	4
28	10 52 33·3	9 57·14	0·93	11 22·6	9·2	18	6	5
Oct. 2	12 1 57·6	7 1·03	1·07	30 19·6	8·4	6	2	6
6	9 54 52·1	4 15·14	0·94	48 48·4	11·5	9	3	7
7	10 30 42·8	3 32·65	0·81	53 41·0	10·2	24	8	8
	11 55 57·7	30·67	1·27	57·7	10·3	21	7	9
8	10 16 27·3	2 52·50	1·13	83 58 21·5	10·6	16	6	10
10	12 43 30·5	1 28·03	0·84	84 8 16·7	9·2	24	8	11
12	10 50 4·5	0 0 13·27	−0·64	84 17 16·3	+ 7·2	24	8	12

Parallax and computed places taken from Suppl. *Naut. Alm.* 1854.

Mean Places of Stars of Comparison, 1851·0.

Name.		R.A.	N.P.D.	Set.
	h	h m s	° ′ ″	
Weisse	0, 318	0 19 13·49	82 15 48·4	1
—	0, 375	22 50·14	18 42·3	2
Green. T.Y.C.	16	12 55·98	82 38 15·3	3
Weisse	0, 169	10 1·75	83 4 7·2	4
Green. T.Y.C.	22	18 1·11	7 58·7	5
Weisse	0, 192	11 27·89	32 45·1	6
—	0, 97	6 4·79	53 20·0	7
—	0, 89	0 5 22·91	55 59·6	8
—	xxiii, 1222	23 59 26·52	83 57 9·9	9
—	— 1252	0 0 53·74	84 2 47·2	10
—	— 1251	0 0 46·54	84 12 46·6	11, 12

Hygeia appeared as a star of 10.11 magnitude.
Sept. 15 and Oct. 2. Interrupted by fog.
Oct. 6. Interrupted by cloud.
— 10. Observed through thin cloud.

IRIS.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1851.	Green. M.T.	R.A.	N.P.D.	Compd—Obsd. R.A.	Compd—Obsd. N.P.D.	Stars of Comp. B.A.C.
	h m s	h m s	° ′ ″	s	″	
Sept. 14	11 16 0·8	0 1 24·04	77 45 43·8	+1·62	−5·7	26, 8331
	11 57 53·7	22·98	49·1	1·36	6·2	— —
15	9 57 31·9	0 0 40·60	77 49 1·4	1·68	7·0	— —
	10 37 25·1	39·54	6·6	1·45	6·2	— —
20	12 11 25·3	23 56 36·19	78 10 47·5	+1·48	−7·2	26

The observed places have been corrected for parallax and refraction. The computed places were deduced from the ephemeris contained in the supplement to the *Nautical Almanac* for 1854.

The following are the assumed *mean* places of the stars of comparison derived from the Greenwich observations:—

	R.A.	N.P.D.
	h m s	° ′ ″
B.A.C. 26	0 5 34·05	75 38 41·02
— 8331	23 51 39·72	83 57 41·82

Durham. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1851.	Green. M.T.	R.A.	Exc. of Ephem.	N.P.D.	Exc. of Ephem.	No. of Comps. in R.A.	N.P.D.	Set.
	h m s	h m s	s	° ′ ″	″			
Sept. 6	13 7 0·0	0 6 50·70	...	77 28 17·8	...	18	6	1
9	10 59 1·8	4 59·83	+0·92	32 35·7	−5·2	24	8	2
	12 16 3·9	57·51	1·09	41·8	5·1	15	5	3
10	11 37 6·1	4 17·90	1·07	34 40·4	3·8	18	6	4
13	11 18 11·5	2 9·28	1·33	42 30·5	3·4	24	8	5
	11 57 17·4	8·18	1·22	34·7	2·7	24	8	6
15	10 59 46·8	0 0 39·22	1·06	77 49 7·0	3·3	24	8	7
28	10 1 19·2	23 50 2·44	1·57	78 56 40·4	1·1	18	6	8
Oct. 2	10 11 52·3	46 50·68	1·31	79 24 22·2	5·7	24	8	9
	10 52 0·1	49·01	1·68	32·9	4·4	24	8	10
4	9 56 33·5	45 19·64	1·64	38 48·9	5·0	9	3	11
6	9 9 8·8	43 54·00	1·57	79 53 28·0	5·1	24	8	12
12	9 49 23·5	23 40 2·78	+1·15	80 39 24·2	−0·4	24	8	13

Parallax and computed places taken from Suppl. *Naut. Alm.* 1854.

Mean Places of Stars of Comparison, 1851·0.

Name.	R.A.	N.P.D.	Set.
	h m s	° ′ ″	
Weisse, 0^h, 128	0 7 52·28	77 24 30·9	1, 2
B.A.C. 8370 (Edin. Obs.)	23 58 3·29	25 57·7	3, 4
Weisse, 0^h, 60	0 3 40·24	46 16·8	5
— xxiii, 1236	0 0 3·19	44 42·7	6
— — 1201	23 58 21·15	77 57 16·2	7
— — 927	45 23·46	78 54 5·9	8
— — 1030	50 9·46	79 21 15·8	9
— — 1000	48 52·24	31 54·3	10
— — 1010	49 22·90	36 36·7	11
B.A.C. 8300 (Edin. Obs.)	45 1·36	79 52 52·6	12
Weisse, $xxiii^h$, 775	23 37 46·56	80 38 46·6	13

Iris shone as a star of 7.8 magnitude, and was exceedingly brilliant on some of the earlier nights in September.

HEBE.

HAMBURG. (M. Rümker.)

1851.	Hamburg M.T. h m s	R.A. h m s	Decl. ° ′ ″	No. of Comps.	
July 3	10 40 55·6	19 26 59·35	— 8 8 30·2	10	Equatoreal
5	10 51 8·9	25 10·11	8 21 57·8	17	—
11	10 8 38·9	19 31·80	9 6 49·2	6	—
12	10 18 5·9	18 33·02	15 2·5	20	—
15	10 58 2·4	15 36·32	40 47·8	6	—
16	11 37 53·8	.....	49 45·0	Meridian Circle	
17	32 59·8	13 38·24	9 58 46·3	—	—
18	28 6·3	12 40·50	10 7 50·0	—	—
21	13 27·8	9 49·16	35 51·0	—	—
22	11 7 35·9	7 52·95	10 45 29·9	—	—
24	10 58 53·4	7 2·07	11 4 59·1	—	—
27	44 26·4	4 22·52	34 55·5	—	—
28	39 38·5	3 30·45	11 45 3·3	—	—
30	10 30 7·6	19 1 50·95	12 5 30·5	—	—
Aug. 6	9 57 28·5	18 56 42·41	13 18 13·6	—	—
8	48 22·0	55 27·59	39 5·9	—	—
9	43 51·5	54 52·94	13 49 31·9	—	—
10	9 39 22·8	18 54 19·99	—14 0 0·4	—	—

These observations are not corrected for parallax.

Mean Places for January 0, 1851, of stars in the track of *Hebe*, from observations with the meridian circle at Hamburg.

R.A. h m s	Decl. ° ′ ″	R.A. h m s	Decl. ° ′ ″
18 41 31·18	— 9 7 0·2	19 13 22·42	—10 38 39·2
51 21·22	13 58 ...	13 33·77	10 40 19·3
53 49·69	13 27 38·5	14 41·50	10 18 43·0
55 0·20	12 55 13·6	15 48·53	9 39 42·8
57 57·21	13 31 14·8	16 14·19	9 37 19·4
58 55·81	12 53 51·0	17 48·60	9 18 9·8
59 1·61	13 3 53·4	19 45·64	9 24 12·4
59 24·03	12 37 35·9	21 44·36	8 19 24·4
18 59 34·18	12 58 30·0	22 30·03	8 44 29·4
19 0 13·74	12 36 21·7	24 22·89	9 19 9·0
4 26·40	11 47 40·7	24 52·74	8 18 37·1
5 0·05	11 14 44·6	27 27·18	7 46 53·9
6 31·28	11 7 17·3	27 33·57	7 38 19·4
6 39·75	10 58 24·9	30 18·71	8 38 47·5
6 38·92	11 9 44·7	31 23·33	7 33 23·5
8 31·52	10 25 53·5	32 25·37	7 57 8·7
11 14·13	10 35 0·4	32 42·21	7 51 2·2
12 4·40	10 13 48·9	19 32 48·58	— 7 58 ...
19 12 11·12	—10 49 54·2		

EGERIA.

*Ephemeris.** By M. G. Rümker.

12^h Berlin Mean Time.

Date. 1851.	R.A. h m s	Decl. ° ′	Log. Δ	Date. 1851.	R.A. h m s	Decl. ° ′	Log. Δ
Nov. 0	10 52 21	+23 15·3	0·42067	Dec. 1	11 40 11	+20 42·7	
1	54 3	9·3		2	41 32	39·3	0·35804
2	55 45	+23 3·4		3	42 52	36·0	
3	57 26	+22 57·5		4	44 11	32·7	
4	10 59 6	51·7	0·41369	5	45 29	29·6	
5	11 0 45	45·9		6	46 46	26·7	0·34918
6	2 25	40·2		7	48 2	23·9	
7	4 4	34·5		8	49 17	21·2	
8	5 42	28·9	0·40646	9	50 31	18·7	
9	7 19	23·4		10	51 45	16·3	0·34010
10	8 56	17·9		11	52 58	14·0	
11	10 32	12·5		12	54 9	11·8	
12	12 8	7·1	0·39900	13	55 19	9·7	
13	13 43	+22 1·8		14	56 29	7·8	0·33083
14	15 17	+21 56·6		15	57 37	6·1	
15	16 51	51·5		16	58 45	4·5	
16	18 24	46·5	0·39128	17	11 59 51	3·1	
17	19 56	41·6		18	12 0 56	1·8	0·32138
18	21 28	36·3		19	2 0	+20 0·6	
19	22 59	32·1		20	3 3	+19 59·5	
20	24 29	27·4	0·38333	21	4 5	58·6	
21	25 58	22·9		22	5 5	57·9	0·31178
22	27 27	18·4		23	6 4	57·3	
23	28 55	14·0		24	7 2	56·9	
24	30 23	9·7	0·37513	25	7 59	56·7	
25	31 49	5·6		26	8 54	56·6	0·30206
26	33 15	+21 1·5		27	9 48	56·7	
27	34 40	+20 57·5		28	10 41	56·9	
28	36 4	53·6	0·36670	29	11 33	57·2	
29	37 27	49·9		30	12 24	57·6	0·29225
30	11 38 50	+20 46·2		31	12 13 14	+19 58·1	

* From Professor Encke's elements, *Berliner Jahrbuch*, 1854.

D'ARREST'S NEW COMET.

Dr. d'Arrest, of the Leipsic Observatory, discovered a faint comet in the constellation *Pisces* on the night of June 27. The place could only be estimated roughly on account of twilight.

	Leipsic M.T. h m	R.A. o ′	Decl. o ′
1851, June 27	13 10	7 49	+10 32

The next night was cloudy; but on the night following Dr. d'Arrest obtained two comparisons of the comet with *Weisse* 0^h, 690, which were as satisfactory as the extreme faintness of the comet would allow.

	Leipsic M.T. h m s	R.A. o ′ ″	Decl. o ′ ″
1851, June 29	13 14 5	10 4 47	+10 37 35

CAMBRIDGE. Northumberland Equatoreal. (Prof. Challis.)

	Greenwich M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$	No. of Comps.	Star.
1851.	h m s	h m s		o ′ ″			
Aug. 7	13 56 3·7	3 7 34·79	−8·561	81 59 5·7	−9·8684	7	*a*
21	12 46 58·7	3 42 34·22	8·597	84 31 7·5	9·8849	6	*b*
Sept. 22	13 6 57·9	4 22 42·55	−8·480	91 41 31·5	−9·9024	8	*c*

The comet on Sept. 22 was faint, and so much diffused that it was difficult to fix upon the brightest point. The following are the assumed *mean* places, 1851·0, of the stars as derived from the Catalogues.

	Mean R.A. h m s	Mean N.P.D. o ′ ″	Name of Star.
a	3 4 36·17	81 58 13·6	Bessel (Weisse), iii^h, 62
b	3 37 45·77	84 25 14·3	*u'* Tauri = B.A.C. 1162
c	4 18 6·55	91 45 16·2	Bessel (Weisse), iv^h, 376

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

	Green. M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$	No. of Comps. in R.A.	N.P.D.	Set.
1851.	h m s	h m s		o ′ ″				
Aug. 24	14 49 36·5	3 48 54·85	−8·426	85 8 54·7	−9·8892	4	4	1
30	13 52 42·2	4 0 21·30	8·499	86 24 44·9	9·8982	4	4	2
Sept. 1	14 30 19·1	2 53·83	8·428	86 51 11·7	9·8972	5	5	3
6	14 33 29·2	4 9 51·97	−8·395	87 59 55·0	−9·9016	8	8	4

P is the equatoreal horizontal parallax in seconds of arc; p and q are the required corrections in time and arc respectively.

Mean places of Stars of Comparison, 1851·0.

Name.	R.A. h m s	N.P.D. o ′ ″	Set.
Weisse, iii^h, 972	3 49 54·30	84 57 0·9	1
— — 1103	3 56 53·49	86 34 24·8	2
— iv^h, 124	4 7 0·25	87 0 42·4	3
— — 180	4 9 32·04	87 50 32·7	4

D'Arrest's comet, as seen here, exhibited no nucleus; the errors of observation will consequently be large. The first observation was taken with a wire-micrometer, the three last with a ring-micrometer, radius 444″. The correction for refraction has been applied. Cloudy weather prevented observation previous to July 18; I was then absent from Durham till August 18. Attempts since Sept. 6 have been unsuccessful.

*Elliptic Elements.**

By M. E. Vogel, from observations on July 2, and July 23, August 4, at Berlin.

M	° ′ ″ 0 0 0·0	1851, July 8·98442, Berlin M.T.
π	323 55 11·7	Mean Equinox, 1851, Jan. 0.
Ω	149 8 17·3	
i	14 10 47·4	
φ	43 33 30·0	
Log e	9·838278	
Log a	0·580140	
Log μ	2·479797	
Periodic Time	2708^{days}·994	(Motion direct.)

Comp^{n}—Obs^{d}.

δ . long. = —0″·2

δ . lat. = —0″·1

By Mr. Norman Pogson, from the following observations:—

1851.	Mean Time at Place. h m s	R.A. ° ′ ″	Decl. ° ′ ″	
June 29	13 4 5	10 4 47·0	+ 10 37 35·0	Leipsic
July 6	13 21 10	17 46 6·6	10 49 20·0	Bonn
24	13 11 49	35 33 50·4	+ 9 53 7·5	—

T	July 10·002868	Greenwich M.T.
π	° ′ ″ 324 7 48·05	Mean Equinox, August 1.
Ω	148 13 14·39	
i	14 4 2·26	
φ	38 31 36·71	
e	0·6228820	
Log a	0·4923758	
Log q	0·0688536	
Log μ	2·8114429	
μ	647″·80271	
Period	2000^{days}·61	

"The middle observation is represented in longitude within 0″·80, and in latitude within 0″·04.

"The periods obtained by those astronomers who have undertaken the calcula-

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* Mr. George Rümker forwarded parabolic elements and an approximate ephemeris; these elements are superseded by the later determinations.

tion of the orbit of this comet are remarkably discordant, as may be perceived from the following comparison :—

Villarceau	2921 days.
Vogel	2709
D'Arrest	2577
— (corrected elements from normal places)	2353
Pogson	2001

"Of these, D'Arrest's corrected elements are, I believe, nearest the truth, and will serve to follow the comet during the remainder of its present apparition.

"With the larger telescopes it will probably be visible for some time longer; and as so much uncertainty exists at present with respect to the time of its return, it is to be sincerely hoped that such valuable observations will be obtained."

BRORSEN'S FOURTH COMET.*

On August 1, at about $11^h\,45^m$ P.M., M. Brorsen, of Senftenberg, discovered a telescopic comet in *Can. Venat.* Though small, he calls it very bright. By seven comparisons with Bessel's star, Zone 413, 8th mag., $13^h\,53^m\,57^s{\cdot}12 + 31^\circ\,31'\,40''{\cdot}2$, he obtained the following place :—

1851.	Senftenberg M.T. h m s	R.A. Comet. h m s	Dec. Comet. ° ′ ″
Aug. 1	13 9 21·4	13 54 58·43	+31 27 7·3
Daily variation		+1 32·9	+ 23 25

ALTONA. (Dr. Petersen.)

1851.	Altona M.T. h m s	R.A. h m s	Dec. ° ′ ″
Aug. 4	11 25 53	13 59 58·93	+32 48 2·6

The air so thick and unfavourable that the comet was seen with difficulty. The observation in declination not to be relied on.

HAMBURG. (M. Rümker.)

1851.	Hamburg. M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
Aug. 4	11 16 11·5	209 59 43·8	+32 48 22·1
5	11 10 0·7	210 26 21·2	33 15 36·5
6	10 47 25·4	210 53 15·7	33 42 25·8
19	10 46 6·3	217 42 25·4	39 51 13·0
21	9 40 52·3	218 55 17·2	40 48 37·7
22	9 36 19·6	219 33 30·4	41 18 1·0
24	9 49 35·1	220 53 51·4	42 18 2·2
25	9 16 51·5	221 36 36·7	42 48 36·4
28	9 20 36·7	223 47 18·1	44 18 52·7
29	10 38 26·2	224 36 27·8	44 51 15·3
Sept. 6	9 37 38·1	231 49 28·2	+48 59 39·7

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* This was also discovered by M. Schweizer at Moskow, on August 21.

1851.	Hamburg M.T. h m s	R.A. ° ′ ″	Decl. ° ′ ″
Sept. 15	9 15 36·6	243 15 27·5	+53 38 42·9
18	9 41 21·3	248 16 46·2	55 5 10·6
22	9 37 10·7	256 5 57·7	56 45 49·9
26	10 32 39·5	265 33 59·0	57 59 30·7
27	9 44 34·8	268 4 24·7	+58 10 22·2

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1851.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	Star of Comp.
Aug. 13	10 18 21·4	14 17 16·87	+8·690	53 1 35·4	−9·7558	B.A.C. 4812
	10 38 20·3	18·29	8·695	1 18·3	9·7788	— —
	10 58 19·5	20·21	8·698	0 52·4	9·8007	— —
15	10 24 47·4	14 21 37·28	8·699	52 4 26·8	9·7605	— —
	10 39 46·4	38·28	8·702	4 14·0	9·7781	— —
	10 54 45·5	39·49	8·703	3 53·7	9·7950	— —
25	9 14 18·0	14 46 23·59	8·700	47 11 56·5	9·6346	B.A.C. 4958
	9 34 17·4	25·45	8·714	31·8	9·6667	— —
26	9 49 11·8	14 49 18·85	8·726	46 40 56·7	9·6831	— —
	10 4 11·5	20·68	8·732	40·6	9·7093	— —
	10 19 10·8	21·93	8·736	17·0	9·7312	— —
Sept. 6	9 53 59·5	15 27 29·46	8·775	40 58 59·6	9·6416	B.A.C. 5177
	10 14 0·8	33·55	+8·781	38·1	−9·6784	— —

The observations are corrected for refraction. The corrections to be applied for parallax in time and arc are represented by p and q. P is the equatoreal horizontal parallax.

The following are the assumed *mean* places of the stars of comparison for 1851·0 :—

	R.A. h m s	N.P.D. ° ′ ″	Authority.
B.A.C. 4812	14 26 4·63	51 2 16·13	Greenwich 12-year Catalogue.
— 4958	14 56 20·07	49 1 9·78	— — —
— 5177	15 33 30·00	42 42 32·01	Oxford and Edinburgh Observations.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1851.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	No. of Comps. in R.A.	N.P.D.	Set.
Aug. 19	11 0 19·0	14 30 54·57	+8·697	50 7 53·0	−9·8100	18	6	1
20	10 57 16·0	33 21·45	8·603	49 38 46·6	9·8058	15	5	2
21	10 9 15·1	35 46·43	8·702	49 10 19·8	9·7474	8	3	3
26	10 45 8·8	14 49 25·09	8·724	46 39 48·5	9·7820	7	3	4
30	10 38 37·1	15 1 43·41	8·739	44 37 17·0	9·7637	14	5	5
Sept. 1	12 52 51·0	8 49·24	8·661	43 31 45·9	9·9034	5	5	6
5	9 56 6·6	15 23 23·45	8·759	41 29 57·5	9·6733	6	6	7
21	10 57 28·7	16 56 41·50	+8·842	33 35 25·1	−9·6429	5	5	8

P is the equatoreal horizontal parallax in seconds of arc; p and q are the required corrections in time and arc respectively.

Mean places of Stars of Comparison, 1851·0.

Name of Star.	R.A. h m s	N.P.D. ° ′ ″	Set.
Lalande, H.C. 2664	114 28 50·47	49 55 1·0	1
— 26804	34 48·25	49 28 3·7	2
Green. T.Y.C. 1182	37 57·50	48 54 29·0	3
Lalande, H.C. 27279	50 32·27	46 36 19·2	4
Green. T.Y.C. 1218	14 57 50·76	44 46 15·1	5
Argel. Z. 118, No. 7	15 4 33·73	43 42 14·8	6
Groomb. 2239 (Johnson)	15 24 38·87	41 46 22·7	7
Argel. Z. 13, No. 68	17 5 4·42	33 38 27·0	8

No nucleus traceable; the comet has gradually been becoming fainter; it was a very difficult object on Sept. 21. I have seen it twice since that date, but have been unable to procure any observation. The ring-micrometer was used on the three last occasions.

CAMBRIDGE. Northumberland Equatoreal. (Prof. Challis.)

Greenwich M.T. 1851. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	No. of Comps.	Star.
Aug. 11 10 44 28·2	14 13 9·68	+8·704	53 57 36·9	−9·7937	6	*a*
18 12 49 45·8	28 41·43	8·652	50 34 47·8	9·9113	5	*b*
28 12 38 28·9	14 55 38·37	+8·686	45 26 21·6	−9·8985	5	*c*

Assumed *mean* places of the stars, 1851·0.

	Mean R.A. h m s	Mean N.P.D. ° ′ ″	Name of Star.
a	14 11 41·56	53 48 2·1	B.A.C. 4747
b	14 26 4·84	51 2 17·0	γ Böotis = B.A.C. 4812
c	14 56 32·15	45 42 17·6	H.C. 27447

The places of the catalogues have been adopted.

*Elements.**

By M. G. Rümker.

Perihelion Passage, 1851, Aug. 26·216505.

Longitude of Perihelion	309 51 7	App^{t} Eq^{x}
— Node	225 1 1	Aug. 4.
Inclination	40 9 24	
Log q	9·999860	

Motion direct.

In the course of the vacation, Lieut. Stratford published an ephemeris of the periodic comet discovered by Brorsen, Feb. 26, 1846, which was expected to return this autumn. The place of the comet is given on three suppositions, that its perihelion passage occurs on Nov. 10, or eight days sooner or later.

* M. G. Rümker was also so obliging as to forward an approximate ephemeris.

Observations made at Paramatta during the Eclipse of the Sun on Saturday, February 1, 1851. By the Rev. W. B. Clarke.

The account given by Mr. Clarke of the observations, of which an abstract is here given, was originally published in the *Sydney Morning Herald*, and has been since forwarded by that gentleman to the Society. The observations were made at Newlands, Paramatta, by Mr. Clarke, in conjunction with Captain P. King. As it is certain that the eclipse was annular at some place near Sydney, and as some doubts are entertained whether certain interesting phenomena that were seen at the time of greatest obscuration were due to the completion of the annulus, or to the near approach to it, Mr. Clarke has appended a plan of the localities in the neighbourhood of the place of observations, from which it appears that his station was the most northerly of all at which observations were taken, but that it probably lay without, though very near to, the northerly limit for the annular eclipse. On the whole, he considers that the annulus was not complete at his station.

The observations made by Mr. Clarke were chiefly meteorological, though he occasionally viewed the progress of the eclipse both through the telescope used by Captain King, which was provided with a red shade, and through another provided with a green shade.

For the meteorological observations Mr. Clarke had Captain King's standard barometer, and an aneroid of his own, which was first compared with it. He had also two thermometers with blackened bulbs, from one of which he had removed the scale by means of a hinge, so as to have the bulb and part of the tube perfectly free; and this he had compared with a standard before commencing. The other had a brass scale unremoved, and both were so placed in a shallow box as to receive the full effect of the sun's rays. Besides these, he was furnished with a wet and a dry bulb thermometer, and with a Daniell's hygrometer.

The meteorological observations we shall classify under the heads, pressure of the air, temperature, radiation, and humidity. The other observations relate to obscurity, effect on vegetation, and the direct phenomena of the eclipse.

Pressure of the Air.—No barometrical results are indicated which show any effect of the eclipse. The small changes in height during its progress are in accordance with the ordinary laws.

Temperature.—The depression of temperature due to the eclipse was found to be $2^{\circ}\cdot82$ at $5^{h}\ 24^{m}$ (the depression for the time of day being allowed for), and this depression continued for about 32 minutes.

Radiation.—The effect on this element was very remarkable. The indications of the brass scale thermometer varied from 111° to 71°, or gave a difference of 40°. The indications of the thermometer with the bare bulb varied from 92° to 68°, giving a difference of 24°. Sudden and frequent variations occurred, which Mr.

Clarke attributes to insensible vapours in the atmosphere. At the time of greatest obscuration the radiation thermometers stood at about the same height as the dry thermometer.

Humidity.—The dew-point, as calculated by the indications of the dry and wet bulb thermometer, varied from 64°·1 to 61°·6, the corresponding dry temperatures varying from 73° to 69°·5. The greatest humidity occurred about the time of the middle of the eclipse.

Obscurity.—Mr. Clarke fancied that the cattle seemed affected by the diminution of light, and he was informed that fowls in some places were preparing for roost. The appearance was that of early morning; but the general hue to the naked eye, at the greatest observation, was that of a pale, sickly green.

Effect on Vegetation. — Immediately beneath the verandah whereon the observation was made grew a plant called the "Four o'clock flower," from the time of the usual opening of its blossoms. Three petals began to unfold, but during the greatest obscuration they closed again; nor did they fairly unfold till the eclipse was nearly over.

Direct Phenomena of the Eclipse.—By Captain King's observations, the eclipse commenced at $4^h\ 6^m\ 6^s$, local time, and terminated at $6^h\ 31^m\ 35^s$. He observed with a refracting telescope of 44 inches focal length, and of $2\frac{3}{4}$ inches aperture, mounted equatoreally, using a power of about 40. He made, during the eclipse, several drawings of the solar spots, and of the different phases of the obscuration.

Mr. Clarke describes the moon as looking spherical and dark, "like a huge cannon-ball, that, having been corroded, is painted black. Its limb was generally far more distinct than that of the sun. A kind of mirage played along the inner edge of the sun's limb, and on the outer edge of the moon, accompanied by undulations. The cusps of the sun were generally most beautifully and delicately defined; but, towards their nearest approach, the points were blunted at one time, and protusions of black interspaces, like slopes of a mountain, divided the light at the angle."

From the near approach to an annular eclipse which took place at his station, Mr. Clarke was prepared to expect something like the phenomenon of "*Baily's beads*;" and when the cusps approached nearest to each other, he was anxiously watching at the telescope. He saw distinctly a formation of the "beads," with "dark bands" between, uniting the edges of the sun and moon near the points of the cusps, just as they are described by Mr. Baily; and he then saw flashes of *white* light rush across the dark portion of the moon from each cusp, but he did not see them unite, though they approached very close to each other.

Captain King saw the junction very nearly all round. Mr. Clarke did not see it all round; but the threads of light were very near to each other; and his impression was that it was not the sun's body, but light glancing rapidly from one summit of the moon to another, "either refractions from the sun, or reflected earth-light: but the

lights certainly came from the direction of either cusp, and nearly touched. This phenomenon lasted but a very few seconds."

Some gentlemen, who threw the image of the sun upon a screen through their telescopes and theodolite-glasses, say they saw the line well defined all round upon the paper. The light, on the contrary, which Mr. Clarke saw, was in rapid projection, quickly disappeared, and was quite *white;* whilst the sun's light in the telescope was that of the *green* screen.

Mr. Clarke's observations of the eclipse have convinced him that, if the moon have any atmosphere, it is of extreme rarity; but, by observations of the eclipses of several spots, he is induced to think that there is *some* atmosphere, however rare.

An Account of placing Meridian Marks for the Stonyhurst Transit Circle. By the Rev. A. Weld, F.R.A.S.

"As the observatory of Stonyhurst possesses the advantage of a well-defined and unobstructed horizon, both in the northern and southern direction, it had long been my wish to fix marks exactly on the north and south points, with which I might compare the position of the transit circle, and so be aware, at sight, of any change that might take place in the bearings of the instrument. I believe the existence of a good meridian mark is always looked upon as an advantageous circumstance in the equipment of an observatory; and since on our horizon there was no object sufficiently defined, and near enough to the meridian, to answer that purpose well, I thought it worth while, to remedy that defect, to determine the exact north and south points of the meridian passing through our transit circle, and to fix marks there which should be easily seen with the ordinary observing power of the telescope.

"I must first state that the north and south points in our horizon are between four and five miles distant from the observatory, the northern being about 500, and the southern 300 feet above our level.

"The first object to be gained was to find the south point approximately, so as to confine our operations within the field of the telescope. This was effected in the following manner: — Two high poles were erected approximately in the meridian line; on the top of one an English, and on the other a French flag was hoisted. The object of these was to guide the exploring party into the right direction: this was known by the flags appearing in the same line. An observer accustomed to the use of telescopes was stationed in the observatory, with an assistant at each flag-post to work the signals, and two others to convey the requisite messages; whilst I went myself, with two or three others, to the hills, provided with a hand-telescope, a small flag, and a staff about eight feet long.

"It was arranged, before starting, that both flags should fly until we appeared on the horizon within the field of the telescope of the transit circle.

"The falling of one of the flags was the signal that we had been thus seen, and were therefore within a short distance of the

point of which we were in search. It was further arranged that as long as we were a little too much to the west the English flag alone should fly; and, on the other hand, the French flag was the signal that we were a little east of the meridian, though visible within the field of the transit.

"The first expedition was made on March 6th, and we succeeded very well in fixing the point approximately. We wandered some time on the hills before our little flag was descried from the observatory; but at length the falling of the French flag satisfied us that we were seen and understood, and the rapid change of the flags as we passed and repassed the meridian convinced us that active and prompt co-operation was afforded us from the observatory. The staff that had carried our flag was driven in, to serve as a mark, until a firmer and more permanent one could be obtained.

"This staff, which was about one inch and a quarter in thickness, was seen distinctly from the observatory (when the day was clear), as a line considerably finer than the spider-lines of the telescope.

"I now determined to remove the angle of collimation of the middle wire of the telescope, so as to make it coincide with the same point on the horizon in both positions of the axes of the instrument. This was done to my entire satisfaction; and I found that whether the illumined axis was east or west, and whether the telescope was turned to the north or south horizon, the middle wire agreed sensibly with the same distant points. This wire, however, does not coincide exactly with the mean of the system of five wires, but the interval between it and the mean has been carefully determined from twenty-six entire transits of *Polaris*, above and below the pole.

"I endeavoured also to reduce, as far as possible, the error of azimuth, in order, once for all, to fix the exact point where our meridian cuts the horizon. At the same time no opportunity was lost of carrying out the operations on the hill.

"It will readily be imagined that the distance, added to the uncertainty of the weather in a hilly country, must have occasioned many failures and consequent disappointments.

"On April 3d, the day being clear, I made a successful attempt. A heavy post of oak was procured, about nine feet in length and eight inches in thickness; this was surmounted by a cross consisting of two bars of iron, each two feet in length and two inches in breadth, hammered together in the middle, and driven by means of two strong spikes into the top of the post. The usual signals were used, with the additional understanding that both flags should fall promptly when the post was so placed that the cross of the bars was bisected by the middle wire of the transit. The signals were seen without difficulty; but I afterwards found from Mr. Vaughan, who superintended in the observatory, that the atmosphere was so unsteady that nothing could be determined with accuracy.

"No convenient opportunity offered itself for another expedition till April 26th, when I dug up the post, and succeeded in

planting it, I believe, with great accuracy in the meridian. On returning home I found the × intersected by the middle wire, but it did not appear to be perfectly bisected by it; the atmosphere was, however, rather disturbed at the time.

"A day or two after, the × appeared to me to be perfectly bisected by the middle wire; and as I have reason to believe the instrument was at that time very nearly in the meridian, I considered the position of the south point sufficiently determined.

"From observations on April 25th, on *α Urs. Maj.* and *δ Hyd. et Crat.*, I calculated the azimuthal error to be $+0''\cdot12$; and on April 29th the mean of the two results obtained from β^1 *Scorpii* and *ξ Urs. Min.* with illumined axis east, and *α Ophiuchi* and *ε Urs. Min.* with illumined axis west, was $-0''\cdot36$.

"So small an arc as either of these, when measured along the horizon, being quite an invisible quantity with the ordinary power of the telescope, I thought there was no use in trying to bring the instrument any nearer to the meridian.

"The only point of interest that remained was to measure the interval along the horizon which corresponded to the interval between two wires, that, knowing the space corresponding to a given arc, I might form an idea of the accuracy of the results which the meridian mark will afford, at the same time that I could calculate the distance from it to the observatory.

"The interval between two of the wires, measured on the ground, was 39 feet, and the equatoreal interval for the same wires $= 24^s\cdot34 = 6'\,11''\cdot10$. This gives 4·13 miles as the distance of the mark from the observatory.

"The northern point was found in the same manner as the south, by expeditions made on April 21st, May 1st, 8th, and 22d, and is marked by a heavy larch-pole, 14 feet in length, and sunk 4 feet deep into the ground. On the top is a cross similar to that used in the south, with this difference, that the bars are 3 feet instead of 2 in length. The distance between two wires, measured along the north horizon, was 45·3 feet, which gives 4·8 miles as the distance from the observatory."

1851, April 10, Mr. Weld observed the occultation of a small star by the dark limb of the moon, at $12^h\,36^m\,59^s\cdot7$ Stonyhurst sidereal time. H.C. 18007.

On some Improvements in Reflecting Instruments.
By Prof. Piazzi Smyth.

In the course of his lectures on Practical Astronomy to the students of the Edinburgh University last winter, Prof. P. Smyth had unusual opportunity of ascertaining those points in the making of the generality of the observations of navigators by sea and of travellers on land, which presented the greatest difficulty to beginners. And as these points generally consisted of needless peculiarities, sometimes absolute imperfections in the instruments, the Professor proceeded to remove them as well as he could, and

the result may, perhaps, be more extensively useful, especially as the difficulties were generally felt on the sextant being applied to observations of *stars by night*, a more exact means than the sun by day, and therefore to be encouraged and assisted in every way.

In naval observations the impediments were, both by the experience of the class, and by the testimony of naval officers,

1st. Difficulty of seeing and of bringing down the star.
2d. Difficulty of seeing the horizon line at night.
3d. Difficulty of reading off the angle on the limb.

The first of these, in so far as it depended on the dark field of the telescope, he proposed to remedy by employing a telescope of large aperture, say 2 inches, in place of the usual size, $\frac{1}{2}$ or $\frac{3}{4}$ inch; in so far as the loss of light was occasioned by reflexion and absorption at the glasses, he intended to remove this by employing *metal* reflectors, by which, too, the occasional nuisance of *second* images would be avoided and greater accuracy obtained. He had tried speculum metal for the purpose with great advantage; but, under some circumstances, he was in hopes of being able to employ silver, which has lately been found to be capable of reflecting near double the amount of light that speculum metal does, though that retains more than quicksilvered glass; and then, in so far as the loss of the star in "bringing down" is caused by the diminished surface exposed by the index-glass at large angles, he proposed to make that larger than usual: besides which, the reflexion taking place in the metal at the first surface, there would be no loss, as now, from the thickness of the edges of the glass or the sides of the brass box containing it.

The second difficulty would be alleviated by the same adoption of the large object-glass: besides the loss of light by transmission through the so-called transparent part of the present horizon-glass would be done away with by the employment of the metal reflectors.

The third difficulty was also shown to be gratuitous, for the reflector of the reading-glass was in general so placed that the light of the lamp could not get to it, and if it did, would be thrown away from the arc instead of on it: and were even that managed, the surface of the vernier and arc being in different planes, the same ray of light would not illumine them *both* at the *same* time. By placing them, however, both in the same plane, and by putting the reflector at an angle of 45° to the limb, instead of parallel to it, so as to receive parallel light and throw it straight down to the divisions, it was found that they could easily be read by a very faint light.

For accuracy, opposite readings were deemed essential, and a circle insisted on in place of a sextant or quadrant; and the author, considering that the failure of the reflecting circle in securing a permanent footing in the navy arose from its being made in general too large and heavy, and complicated, he had devised a very small, but strong and simple form; the telescope was more firmly connected by moving in grooves on the large surface of the face of the circle, instead of rising by the usual single screw; and in place of the inconvenient plan of having to reverse the hands so

as to put the instrument into its box face uppermost, (which makes the getting of it out again without pulling at the reflector or some such delicate parts, difficult) by placing the legs not on the back but on the face, the instrument may be either put into its box, or down on the floor, or anywhere, face first, with the same hand which was moving it in the observation, with the divisions and the reflectors protected from all accidents, and the whole instrument ready at any time on a moment's notice (for the telescope never need be taken off, with its improved fixing), to take advantage of an instantaneous opportunity of observation.

So much for the use of the reflecting instrument at sea: as used on land, the following difficulties were found, and are generally recognised:—

1st. The impossibility of measuring in the mercury either sun or star when within, say 20° of the zenith, from the reflecting instrument not taking in so large an angle; and again, when within, say 10° of the horizon, from the foreshortening of the reflecting surface.

2d. The difficulty of seeing the referring point all night, viz. the reflected image of the star, when black glass is employed; and the trouble with wind when employing mercury, as well as with other causes producing vibration; and the great weight and liability to loss in long journeys through difficult and uncivilised countries.

All these difficulties seemed to be met by making the reflecting surface of speculum metal, levelled by a spirit-level; and when the reflected object could not be seen, attaching to the metal a collimating telescope, whose optical axis was parallel with the previously levelled surface, and was defined at the focus end by a horizontal slit, illuminated by a lamp at night, so as completely to remove all difficulty of seeing the referring object, and allowing of almost the whole object-glass being brought to bear on the star.

Difficulty having been found by the students in keeping sight of an object reflected from the artificial horizon, the latter was generally placed on a stand so as to bring it near the eye, and make it thereby offer a large angular space, which was pretty sure not to be exceeded by the shaking of the hand or involuntary movement of the head of an unpractised observer; but it was found requisite, not only to make the stand firm, but to improve the steadiness of the levelling screws, which was done by making them parts of a fixed frame, with the reflector moveable on them, and capable of being fastened in any position between opposite nuts.

A sextant with all the improvements (except the opposite readings), a full-sized model of a circle, and one of the reflecting horizon, were shown; but Prof. Smyth did not mean to claim any part of them as his own invention; for, without making any special inquiries as to how far he might have been preceded by any one else, he believed that he had only brought to bear on this subject individual improvements long and well known in other departments of the science: but as they had never, he thought, been so com-

pletely united before, and as such a reunion might enable observations often to be obtained when now they are given up, he hoped that the communication might not be uninteresting to some of the numerous working members of the Society.

Bond's Ring of Saturn. Extract of a Letter from Mr. Hartnup.

"On the 15th September we had here one of those superb nights with which we are so seldom favoured. I never before saw *Saturn* so beautifully defined. The main division in the bright ring was steadily seen in every part, except where the ring was hid by the belt of the planet.

"Bond's ring could not be overlooked for an instant. I examined it carefully with powers from 170 to 1133, with all of which it was seen, but most distinctly with 404 and 606. The new ring is much broader than represented by Prof. Bond and Mr. Dawes when first discovered. It occupies full half, or from half to two-thirds of the space between the inner bright ring and the ball of the planet. It has a mottled appearance, is most distinct near its junction with the inner bright ring, and fades away as it approaches the ball of the planet."

M. C. Rümker has forwarded transits of the moon and moon-culminating stars over the meridian of Hamburg, for the following days:—

1850.

Jan. 20	May 22	Aug. 18
21	23	19
Feb. 22	June 17	20
April 18	19	Sept. 12
19	20	15
22	22	16
May 19	July 18	Oct. 13
20	21	Nov. 12
21	22	

1851.

Jan. 11	Feb. 13	June 6
14	23	8
15	Mar. 10	10
Feb. 7	14	11
10	May 10	July 11
11	12	

which are deposited in the archives for the use of those who may want corresponding observations.

On August 16, M. Hencke of Driesen observed a star in *Aquila*, of the 9th mag., which he had hitherto not seen in that place, and which, therefore, was probably a new planet or a variable star. The position for 1800 is,

R.A. $20^h\ 5^m\ 18^s$ N. Dec. $8^\circ\ 30'$

As Dr. Petersen found the star in the same place on August 19, it is probably a variable star.

*Extract of a Letter from the Rev. John Slatter, F.R.A.S., Rose Hill, near Oxford, June 26, 1851.**

"There was an occultation of *Jupiter's* fourth satellite on June 20th, at about $16^h\ 30^m\ 30^s$ Sid.Time, Rose Hill, not mentioned in the *Nautical Almanac*. The lowness of the planet prevented my making a satisfactory observation. I estimated that the centre of the satellite would pass about 1″ below the apex of the planet.

"I have made the following observations of *Jupiter* and his satellites :—

Sidereal Time, Rose Hill.

1850.			h	m	s		
March	17	1st Sat.	8	25	9	Internal Contact.	Egress of Shadow.
		—		30	4	External —	
April	17	3d Sat.	10	12	0	Inter. —	Ingress of Shadow.
	23	1st Sat.	11	14	45	Exter. —	Ingress of Satellite.
		—		20	45	Inter. —	
May	9	1st Sat.	11	40	40	Inter. —	Ingress of Shadow.
June	1	1st Sat.	14	28	28	Exter. —	Egress of Shadow.
	2	2d Sat.	13	19	46·5	Estim. Centre.	Ingress of Satellite.
		—		22	16·5	Inter. Contact.	
1851.							
June	20	1st Sat.	14	26	37	Estim. Centre.	Ingress of Satellite.
		—		29	55	Inter. Contact.	
		1st Sat.	15	41	33	Estim. Centre.	Ingress of Shadow.
		—		43	38	Inter. Contact.	

The power used in all the observations was 236.

"The following observations were made on the relative brightness of the satellites. To avoid bias, I made a diagram, affixing to each satellite one of the four first letters in the alphabet, and put these down in the order of brightness before consulting the *N. A.* to identify them. The observations were made with full aperture and various powers, generally 120, 180, and 236. The order set down was the result of the scrutiny with all the powers, aided, when possible, by another eye; the judgment of which, taken independently, usually coincided with my own. The result may, therefore, be taken as a pretty accurate expression of what the instrument indicated at the time and place of observation. I must observe, however, that the atmosphere will be found to have a great effect on the observation; any slight haze is sometimes enough to surround the planet with a halo of false light: in this case a high power was sometimes of great service (though higher than the night could bear for purposes of clear vision), in order to separate the satellites far enough from the planet to admit of their being brought into the field for comparison without the planet.

"In thirty-eight days' observations, on twenty-four of which all four satellites were observed, the result was to place the order of

* The telescope at Rose Hill is an achromatic of 4·9 inches' aperture and 7 feet focal length, by Simms, mounted equatoreally.

Latitude of Rose Hill, $51^\circ\ 43'\ 50''$ N. Longitude, $0^h\ 4^m\ 56^s{\cdot}5$ West.

brightness thus, 3, 1, 2, 4, on fifteen days; 3, 2, 1, 4, on nine days. There did not appear to be any periodicity in the changes of the first and second. In the series there were two instances of the remarkable diminution in brightness which the third always suffers in transiting the disc of the planet. June 6 it is noted as darker than the belts. With these exceptions it was always noted as the brightest. The fourth was always at the bottom of the scale, though noticed as comparatively much brighter at the period of greatest elongation; and this could not arise from contrast with the light of the planet, as the others would have been affected similarly (*e. g.* on the 20th of June last, the fourth and first were noticed at equal distances from the planet; the first much brighter than the edge it was approaching, the fourth remarkably dull). I think it will be found that there is a periodicity in this satellite's brightness answering to the line of quadratures and conjunctions. I only noted one exception to this, 1850, May 10, when all satellites were near *Jupiter*, and the order appeared 3, 4, 1.

"This method of observing by mutual comparison without reference to an absolute standard is, of course, very unsatisfactory, but something might be done by multiplied observations.

"On June 8, 1850, at 10 P.M. I observed a phenomenon which I had never seen before,—the whole of the north pole down to latitude 40° covered with a dark cloud of the same colour as the belts.*

"If, as there is every reason to suppose, *Jupiter* is covered with a gaseous envelope, is it not possible that the belts mark the situations where the ascending equatoreal and polar currents mix, and which may, in consequence, be incapable of reflecting light? The slight inclination of *Jupiter's* axis to his orbit would then account for their permanency."

Elements of the Elliptic Orbit of γ Virginis.

By Isaac Fletcher, Esq.

"The following elements of the orbit of *γ Virginis*, were computed by Sir John Herschel's process, described in vol. v. of the Society's *Memoirs*. The observations used in the calculations are those compiled by Sir John in his volume of observations at the Cape, together with those which immediately follow, and which depend on my own determinations.

Epoch 1850·364	Position 176° 40′	Distance 2″·946	}	Tarn Bank Observations.
— 1851·401	— 175 58	— 3·047	}	

Elements.

Perihelion Passage	1836·381
Inclination	28° 41′
Position of Ascending Node	19 32
Angle between the Lines of Nodes and Apsides	298 22
Excentricity	0·8876
Period	185·71 years.

"These elements represent the observations very well."

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* An explanatory drawing was sent.

On the Construction of Standard Thermometers.

By Mr. Sheepshanks.*

On considering the different stages of a complete and satisfactory solution of the problem which has engaged me so long, viz. how to fix the standard yard, and to multiply trustworthy copies of it, I was very early led to inquire, as a necessary part of my task, into the reliance which could be placed on such thermometers as were in ordinary use or easily procurable. My very constant friend and coadjutor, Mr. Simms, gave me every information in his power; but very frankly admitted that he did not profess to make thermometers possessing the degree of accuracy I seemed to require, and that, to the best of his knowledge, there were no thermometers to be had which would at all come up to my mark. After a little inquiry and some consideration, in which I received much valuable information from Professor Miller, I came to the conclusion that if I wanted to have thermometers on which I could thoroughly rely, I must make them, that is, *graduate* them, myself.

I now proceed to explain the method which I have adopted to produce a standard thermometer; and though the trouble is great, and some doubts perhaps hang still upon a part of the process, I believe I can produce, with tolerable certainty, instruments which, with certain precautions, will show a thirtieth or even a fiftieth of a degree of Fahrenheit. I only require that skill in the workman which is usually to be found, and I do not *depend* upon him at all.

The two forms of thermometers which I graduate are those which have tubes divided into 180 or 120 equal parts; the divisions are bit with fluoric acid on the glass. The divisions which I use are a tenth of an inch apart, and they are drawn on the wax covering the tube by Mr. Simms' straight-line dividing engine. It is desirable, though not strictly necessary, that the divisions should be equal, and it is also desirable that the bore of the tube should be pretty uniform. The maker will, "for a consideration," select the tubes; and it is worth while to pay the additional price, as it saves a great deal of trouble. I do not know whether the circumstance was accidental, but I found some round bores almost perfectly true: this has never been the case with the flat bores. The latter, however, are much more easily read. The flat bore must have no sensible *twist*.

The bulbs should not be very small or very large: I find from three or four tenths to half an inch or a little more in diameter is a convenient size. In my longer tubes, those of 18 inches, I direct the maker to have the freezing-point as near the zero of division as he can safely get it, only it must be *above*.† The boiling-point must also be within the graduations, and the nearer 180^{d} the better.

* The substance of this account was delivered orally at the June meeting, when the apparatus was exhibited.

† The tubes may be divided before the bulb is blown, but this is not necessary.

I call these, *generating* thermometers; for, as will be seen hereafter, no thermometer is constant in *wide* ranges and at *different* times. The divisions in a well-contrived thermometer of this kind are, of course, rather greater than degrees of Fahrenheit, but not much.

The thermometers which are divided into 120 equal parts may have the freezing-point either high or low, according to their proposed use. For ordinary use, in this climate at least, I should wish to have the freezing-point somewhere about 10 divisions, and to have the 120 divisions equivalent to about 100° of Fahrenheit. I have reason to believe that, even at this temperature, the bulb of the thermometer suffers a small change, which continues for some time, though not permanently. For nice observations at usual temperatures, the freezing-point might be brought very near the zero, and the upper end of the scale made to represent 90°, or even 85° Fahrenheit.* It is better to have special instruments for special purposes, and, if extreme cold is to be measured, to have one instrument for this service.

The first and indeed only essential manipulation to learn, is that of *breaking the column* of mercury. On holding the bulb uppermost, the mercury will run down to the farther end of the tube, and a *vacuum bubble* will show itself on the bulb. If the tube be now quickly and adroitly reversed, the vacuum bubble may be driven into the *neck* of the bulb; and if that is done, a slight tap when the thermometer is horizontal will break off the column of mercury *at the neck* of the bulb. With a little practice this trick is easily learned, and by warming or cooling the bulb, the length of the column to be broken off can be adjusted very nicely to what the experimenter requires. By repeated trials, I can always break off a column to a quarter of a division, or even closer; but this accuracy is more than sufficient, if the tube is pretty regular.

The column should *unite* again, by holding the tube inclined and the bulb downwards; or the mercury may be driven from the bulb to the other end by a smart stroke on the palm of the left hand, with that end downwards, while holding the thermometer in the right, bulb uppermost. If now the bulb be lowered, keeping the vacuum bubble *out* of the neck, the column should unite.

From some causes which I cannot explain — perhaps from little irregularities at the neck of the tube, foulness or imperfection in the tube itself, or impurity in the mercury — it frequently happens that the mercury will either not *break*, or will not *unite*, as it should do. I should recommend the rejection of a thermometer, *for the purpose of a standard*, where these contradictions are frequent, and where the instrument seems refractory. But I have always been able, in the end, to obtain my purpose, though sometimes at a considerable expense of time and temper.†

* A small chamber is or may be contrived at the top of the tube, to prevent breakage by extraordinary expansion.

† Bessel seems not to have known this art of breaking the column, for which I am indebted to Mr. Frankum. Bessel's plan of separating the column by the flame of a lamp is very unsafe. There is the pressure of an atmosphere on the

Supposing this manipulation acquired, and a tractable thermometer provided, having a division upon the glass stem to 180 or 120 equal parts, I proceed as follows. First, with the tube having 180 divisions I break off a column, which, when the tube is horizontal, reaches from 0^d to about 90^d; let us suppose to $89^d{\cdot}4$. I now bring one end of this column to 180^d, and read the other, which suppose $89^d{\cdot}8$. As the same mercury is used in the two measures, it is clear that the *capacity* of the tube from 0^d to $89^d{\cdot}4 =$ the capacity from $89^d{\cdot}8$ to 180^d, or that the mean of the two readings $89^d{\cdot}6$ will be the point which bisects the capacity of the tube from 0^d to 180^d. As $89^d{\cdot}6$ on the tube in question would be expressed by 90^d on a *perfectly equal* tube, the *correction* to $89^d{\cdot}6$ or to 90^d is $+0^d{\cdot}4$.*

The *length* of the column, *i. e.* the length it would occupy if the tube were uniform, is 0^d 2 short of half-way, or is $= 89^d{\cdot}8$.

Before proceeding to a new breaking of the column, I carry one end of the present column to 60^d, and read the other somewhere near 150^d; and after that one end to 120^d, and read the other near 30^d. The use of these will be seen immediately. I now cool the bulb, if necessary, and break off a column which is about equal 60^d, and this I carry forward at three steps from 0^d to 180^d, *i. e.* I make one end fall successively on 0^d, 60^d, and 180^d, and read the other end.

It is clear, that if the error of the division of 60^d tends to lengthen or shorten the first of these measures, it will tend to shorten or lengthen the second of these measures, and in like manner the error of the division of 120^d will have *contrary* effects on the second and third measures; that is to say, the *sum* of the three measured lengths will not be affected by an error in the divisions 60^d and 120^d, and therefore the *mean* will give the true length of the column, in parts of a *perfect* tube of the same capacity. Suppose this mean to be $60^d{\cdot}3$, then the difference of the end of the first reading from $60^d{\cdot}3$ will be the correction to 60^d, and the difference of the third reading from $180^d - 60^d{\cdot}3$, or $119^\circ{\cdot}7$, will be the correction to 120^d. These readings, as well as the former, should be repeated until there is no doubt of their exactness. I now carry the column forwards and backwards from 90^d, and read the ends towards 30^d and 150^d.

softened glass, which, without caution, might strangle the tube. When the column is broken above the neck of the bulb, sometimes a union may be effected by turning the whole body of mercury up and down, backwards and forwards. Sometimes the bulb may be cooled, so that the separation enters into the bulb and becomes obliterated. By heating the bulb and cooling it, the larger mass will seem to lick up the fragment. This last process is of no importance while dividing the thermometer; but as heat above 100° will disturb for a time the freezing-point of an established thermometer, this mode of uniting the column is only to be resorted to in case of necessity, and the thermometer should be laid aside for some time to return to its original state.

* On the supposition that the tube is correct enough to permit the assumption that the error *near* 90^d does not sensibly differ from the error *at* 90^d. This assumption is made throughout: but if the tube is very unequal, special precautions must be resorted to.

Now the *true* value of 90^d being known, viz. $90^d{\cdot}4$, and the true length of the column of 60^d being also known, viz. $60^d{\cdot}3$, it is clear that the *true* value of the other ends of the column, after the column of 60^d has been carried backwards and forwards from 90^d, will be $90^d{\cdot}4 - 60^d{\cdot}3$ and $90^d{\cdot}4 + 60^d{\cdot}3$, or $30^d{\cdot}1$ and $150^d{\cdot}7$; and as the *actual* readings are known, the difference from the *true* values will give the *corrections* of 30^d and 150^d. In like manner, since the *corrections* of 60^d and 120^d are known, and the value of the column of 90^d is also known, the column of 90^d carried back from 120^d and carried forward from 60^d will give the *true* values of 30^d and 150^d, and, when compared with the *actual* readings, will give the *corrections* of those two divisions. Consequently, the *corrections* of the divisions 30^d and 150^d are given by two independent methods; and if these agree well, the *mean* must be correct. If there is any discrepancy, the operations must be repeated *toties quoties*, until a satisfactory conclusion is arrived at, though with moderate care and a steady temperature* no error should occur.

I now break off a column of 50^d and ascertain its *true* length, *i. e.* what it would measure in a perfect tube of the same capacity, by carrying it thrice from 0 to 150^d and thrice from 30^d to 180^d (the corrections of 150^d and of 30^d have already been ascertained). If these two values of the length agree, I have got, just as with the column of 60^d, the errors of 50^d and 100^d, and of 80^d and 130^d. I now carry this column backwards from 60^d, which gives me the error of 10^d; backwards from 90^d, which gives me that of 40^d; backwards from 120^d, which gives me that of 70^d; forwards from 60^d, which gives 110^d; forwards from 90^d, which gives 140^d; forwards from 120^d, which gives 170^d. There are now only two of the decades missed, viz. 20^d and 160^d. These are got by carrying 50^d backwards from 70^d and forwards from 110^d, but as these are derived secondarily, it is prudent, in a former stage, to carry the column of 60^d backwards from 80^d and forwards from 100^d. If the two independent values agree nearly, a mean may be taken, and there can be little doubt of the result.

By breaking off a column of 45^d and carrying it four times along the whole distance from 0^d to 180^d (or from the two extremes and from 90^d), its *length* may be obtained. Carrying this backward from 50^d, 60^d, &c. the corrections of 5^d, 15^d, &c. may be ascertained, and by carrying it forwards from 0^d, 10^d, 20^d, &c. the corrections of 45^d, 55^d, 65^d, &c. are obtained. The corrections of the divisions into fives from 45^d to 135^d are thus got twice over, and independently, which is a great confirmation, and the accuracy of the mean is much increased.

When the tube is tolerably good, *i. e.* when the errors of the divisions follow a sort of law, I have found a careful interpolation of the corrections of the tens sufficient, without actually measuring

* The expansion of the column at first frequently plagued me; but when this was allowed for I have had no trouble, except from mere blunders of reading off, or in arithmetic.

those of the fives. This is a great saving of trouble, and can scarcely mislead, if from each ten the interpolation is made for the preceding and succeeding five.* Should the double values agree pretty well, the mean cannot be far wrong.

When the tube is divided into 120 parts, the only difference is, that the columns first broken off are of 60 and 40 divisions respectively: by means of these the corrections of 60^d and of 40^d and 80^d are first obtained, and also the corrections of 20^d and 100^d. The length of the column of 50^d, which is now broken off, is ascertained by carrying it at twice from 0^d to 100^d, and again by carrying it at twice from 20^d to 120^d; thus the errors of 50^d and 70^d are determined. With the known length of the column of 50^d and the known corrections of 40^d, 60^d, and 80^d, the corrections of 10^d, 30^d, 90^d, 110^d, 130^d, are determined. If an interpolation leaves some uncertainty as to the divisions into fives, a column of 45^d is run through the whole tube by steps of five divisions. The *length* of this column is derived from the known errors of 0^d and 90^d, of 10^d and 100^d, of 20^d and 110^d, and of 30^d and 120^d; and with this value and the known corrections of the decades, those of the pentades are deduced.

If the observer is careful to assure himself that each stage of the operation is correct before proceeding to the next (and the verifications almost force themselves upon him), the work will proceed to the end without any vexatious harking back. The subdivision can be carried below the fives, but this is scarcely ever necessary. It is better to reject a manifestly imperfect tube than to employ so much time as would be necessary to correct each

* I scarcely know whether the mode of interpolation is commonly known; I made out the formula more than twenty years ago, and have used it with great satisfaction. The quantities to be interpolated are written in a vertical column, and the differences taken as usual. The *interpolating* coefficients for each of the original quantities is computed as follows:—

$$
\begin{aligned}
A = {} & \tfrac{1}{2} \text{ sum of 1st differences above and below the horizontal line.} \\
& - \tfrac{1}{12} \text{ sum of 3d differences} \quad — \quad — \quad — \\
& + \tfrac{1}{60} \text{ sum of 5th differences} \quad — \quad — \quad — \\
B = {} & \tfrac{1}{2} \text{ 2d difference on horizontal line.} \\
& - \tfrac{1}{24} \text{ 4th difference} \quad — \quad — \\
C = {} & \tfrac{1}{12} \text{ sum of 3d differences above and below the horizontal line.} \\
& - \tfrac{1}{48} \text{ sum of 5th differences} \quad — \quad — \quad —
\end{aligned}
$$

which are sufficient for most purposes; then if $\mp x$ be the required fraction of an interval above or below one of the original quantities as M,

$$
\begin{aligned}
\text{for } - x \text{ the interpolated value} &= M - Ax + Bx^2 - Cx^3 \\
+ x \quad — \quad — &= M + Ax + Bx^2 + Cx^3
\end{aligned}
$$

the interpolation where $x = \frac{1}{2}$, or $\frac{1}{4}$ or 0·1, 0·2, &c., is very rapidly performed, and the checks so satisfactory, that error can scarcely creep in.

single division, though I have done it very satisfactorily in a tube which had a manifest strangulation at one part.

I have described the process as it may be conveniently performed *by eye*, and it is desirable to examine each tube in this way, even when a more careful determination is proposed. I do not think that by mere estimation a tube can be divided with greater accuracy than 0·1 div., though this will depend on the observer. But even this degree of accuracy is beyond what is found in many standard thermometers, and such an examination costs very little time, not, I think, so much as half an hour.

In dividing my own thermometers, I have always read off each end of the column by a short telescope, which is carried by a slide parallel with the tube, the tube itself resting horizontally in two notches. I find the indistinctness of the end of the mercury and the division on the glass is not troublesome, when the distance from the tube to the object-glass is about four times that from the object-glass to the image. If the telescope is not longer than 5 inches, the tube can be touched without disturbing the observer. The micrometer should have a large divided head, and one revolution of the micrometer should correspond nearly to one division on the tube. Thus hundredths of a division are read at once from the micrometer, and thousandths may be estimated.* By moving the telescope nearer to and farther from the object, and by pulling out and pushing in the eye-piece, it would be easy to adjust the micrometer revolution to the division of the tube; but tubes are seldom quite straight, and I find it best to place the fine wire (which is used for observing the end of the column and the preceding and succeeding divisions) upon the division preceding the end of the column by a slow motion in the slide, when the micrometer reads 0; the fine wire is then carried by the micrometer to touch the end of the column, and afterwards to split the succeeding division, both of which readings must be noted. The observations are then repeated and noted, moving the micrometer backwards. Taking for the true reading the mean of the forward and backward readings in each case, the difference of the readings of the preceding division and of the end of the mercury, divided by the difference of the readings of the two divisions, gives the decimal of a division required. A short table of double entry will save trouble in computing when the adjustment is tolerably nice. The remaining computation is the same as in the eye estimation.

An example of the calculation of the errors of every ten divisions in a thermometer graduated to 120 equal parts, will show the operation better than any mere description.

* In my own apparatus, the screw is too fine and the telescope too long, so that a good deal of time is lost in making the observation and in computing the decimals of the division. One division on the tube corresponds to four revolutions of the screw, so that there is a chance of setting down a wrong number of revolutions, a chance which not unfrequently happens.

The readings of the divisions corresponding to the ends of the column, reduced from the micrometrical observations, are as follows :—

Column = 60^d ±.

^{d}	^{d}	^{d}	^{d}
— 0·294	60·118	19·628	39·900
60·276	119·706	79·777	99·811

Column of 40^d ±.

^{d}	^{d}	^{d}	^{d}	^{d}
0·136	40·147	80·390	19·806	60·212
40·111	79·653	119·600	59·590	99·530

Column of 50^d ±.

^{d}	^{d}	^{d}	^{d}	
— 0·060	50·218	20·139	70·435	
49·982	99·524	69·852	119·559	
^{d}	^{d}	^{d}	^{d}	^{d}
10·209	30·125	40·177	60·299	80·343
60·088	79·682	89·612	109·511	129·374

The — signifies that the end of the column is below the zero.

The operation is nearly the same throughout. The *length* of the column, *i.e.* that which it would occupy in a *perfect* tube of equal capacity from 0^d to 120^d, is first determined; and this column, having one end near a division which has no correction or a known correction, measures the error and correction of the division near its other end.

Apparent values of column of 60 ± = 60·570, = 59·588 } By first pair of observations of 60 ±.

Mean or *true* value of col. = 60·079

Apparent values of column of 40 ± = 39·975, = 39·506, = 39·210 } By first three observations of 40 ±.

Mean or *true* value of col. = 39·564

Correction of 60 Division.

Reading of *known* end	— 0·294	119·706
True length of column	+ 60·079	— 60·079
Actual value of other end	59·785	59·627 *i.e.* what each *should be.*
Corresponding reading	60·276	60·118
Correction to 60^d	— 0·491	— 0·491 for scale of equal parts.

Correction of 40 and 80 Divisions computed.

Reading of *known* ends	0·136	119·600
True length of column	+39·564	−39·564
Actual value of other end	39·700	80·036
Corresponding reading	40·111	80·390

Correction to 40^d = −0·411 to 80^d = −0·354

The middle pair of readings may be used to check these corrections.

Corrections of 20 and 100 Divisions computed.

	By Column 60^d·079.		By Column 39^d·564.	
Readings	79·777	39·900	59·590	60·212
Corrections	−0·354	−0·411	−0·491	−0·491
True value	79·423	39·489	59·099	59·721
Column	−60·079	+60·079	−39·564	+39·564
Actual value	19·344	99·568	19·535	99·285
Corresp. reading	19·628	99·811	19·806	99·530
Corrections	−0·284	−0·243	−0·271	−0·245

The means may be taken, though the corrections of 20^d are not satisfactory. We have,

Correction 20^d = −0·277
100 = −0·244

These are the first or fundamental corrections, and the observer must not proceed until these agree satisfactorily.

Length of column 50± computed.

The readings of 100 and of 20 being corrected according to what has just been found, we have, for *apparent* values of this column,

First pair { 49·982 + 0·060 = 50^d·042 ; 99·280* − 50·218 = 49·062 } Mean = 49^d·552

Second pair { 69·852 − 19·862* = 49·990 ; 119·559 − 70·435 = 49·124 } Mean = 49·557

The *length* may be safely assumed = 49^d·554.

* These values are *corrected* for the errors of 100^d and of 20^d.

Corrections of 50 computed.

Reading	= − 0·060	Reading	99·524
Column	= +49·554	Correction	−0·244
Actual value	49·494		99·280
Corresp. reading	49·982	Column	−49·554
Correction	−0·488	Actual value	49·726
		Corresp. reading	50·218
		Correction	−0·492

Mean or correction of 50^d = −0·490.

In like manner the corrections of 70 = −0·436 and −0·430;
mean = −0·433.

Now, *correcting*, in the remainder of the readings of 50, those which are known already, and adding or subtracting $49^d \cdot 554$, we shall have the *true* values as well as the actual readings, and the following corrections will be found :—

	d
For 10	−0·166
30	−0·351
90	−0·292
110	−0·149
130	+0·169

On writing the whole series down and taking the differences, it appears that interpolated values of the fives agree pretty well whether they are derived from the preceding or succeeding ten. If not, or if I wished to be *quite certain*, a column of 45^d must be broken off and treated as before directed.

I like to have the two ends of the mercury nearly symmetrically placed with respect to the divisions which are to be corrected; but if the tube is tolerably even, no sensible error is likely to arise from slight neglect.

It has been assumed that a column of 40 divisions can be broken off, and this is almost always practicable with the generating thermometers, unless the freezing-point is very high. But in the other thermometers, it is not always possible to get so short a column, except in winter. Temporary means, such as dropping ether on the bulb, may be resorted to at times, but the inconvenience is considerable; a set of observations may be easily lost by the junction of the mercury, and the temperature and consequently the length of the column is variable. If a thermometer were required to show severe cold, I should *graduate* the tube with a smaller amount of mercury. The bulb might then have the proper supply of mercury added, or a fresh bulb might be blown and filled; the *graduation* of the tube is not affected. The same course must be pursued if a bent tube thermometer is wanted; at least I should despair of graduating such a tube, owing to the difficulty of breaking and

uniting the column,* though perhaps a more persevering experimenter would succeed.

It will be seen, that though I have explicitly laid down the necessity of having the divisions cut upon the tube itself, there is nothing in the directions here given which might not be applied to divisions on a *scale*. But a thermometer so mounted is far more dangerous and difficult to manipulate, and there is a perpetual risk that the relative position of the tube and the scale may be altered. For most purposes, the usual form is sufficient and more convenient; but the object I have had in view was to obtain the utmost attainable accuracy with the least probable risk, without regard to the trouble of the maker or of the future experimenter.

The illustrious Bessel has given in Poggendorff's *Annalen*, vol. lxxxii. p. 287, an account of the method he employed to verify his standard thermometers. I own that I very much prefer my own method, which is, I think, more systematic and more certain, checking itself as it proceeds. I made trial of Bessel's plan with respect to a thermometer which had its freezing-point too high for my mode of operating, and got very fair results. Something of his mode may be applied thus:—In a thermometer which has had its principal divisions *measured* as above described, and the errors of the other divisions assigned by *interpolation*, break off a column *ad libitum* and carry it at certain intervals from one end to the other of the tube. Determine the mean *length* of this column, using the interpolated values of the errors of the divisions, and then this known length and the *measured* divisions will give safe values for the errors of the other divisions.

The thermometer being thus carefully subdivided, and I think it practicable almost to insure the second decimal, the next thing is to ascertain the freezing and boiling-points of the *generating* thermometers. It has been known for some time that in a new-made thermometer, which is usually sealed hermetically while the mercury is boiling, a considerable time must elapse—six months or thereabouts—before the bulb arrives at its permanent dimensions. It has not been as yet worth my while to ascertain the exact amount of this *contraction*, as my concern has only been with the points of *freezing* and *boiling* water, but it is necessary that this *retraite*, as it has been called, should have taken place before any exact experiments are made to determine the fundamental points of the thermometer. The graduations may, of course, be examined and corrected previously.

I determine the freezing-points by laying the thermometers *horizontally* in pounded ice, *which is carefully drained*. I have hitherto used the ice which was found on some tubs filled with

* Of four thermometers which had their tubes bent after graduation, two were quite useless, as the mercury always broke at the neck when the temperature was lowered. Two others fared better, and have rendered me excellent service in testifying to the temperature of the mercurial trough in which my standard bars are suspended and supported, with an almost perfect agreement.

rain-water, and I found experimentally the same results precisely from this ice and from new-fallen snow.* But I propose to use the ice of Wenham Lake in my future determinations. It is so much more convenient to perform the manipulations when the ice is melting very slowly, that I should not willingly determine freezing-points except in winter. To make sure that the ice is not *below* the freezing-point, it is kept in a vessel with water in a room above the freezing-point. The ice is very lightly heaped over the bulb; care must be taken to avoid any pressure, and the *horizontal* tube is read with a *vertical* telescope. It is more easy to prevent the possibility of *parallax* this way than in any other which can easily be put in practice; and unless the experimenter is very careful as to the dryness and light pressure of the ice, the horizontality of the tube, and the verticality of the reading telescope (which I need not say is fitted with a micrometer, and is used exactly as has been described already in reading the divisions), the results will be far less accordant and satisfactory than they might be.

The settling the boiling-point is not so easy as that of the freezing-point, and at first I utterly despaired of getting anything like a trustworthy determination; but working, and I think improving, on Professor Miller's instructions,† I at last succeeded even beyond my expectations.

My boiler is made of sheet copper, square above and cylindrical below; the dimensions, which are not very material, are, length 24 inches, breadth and depth about 6 inches, with flat ends and top. In one end there is a round hole, fitted with a large cork; through the centre of the cork a small pipe of copper pierces, large enough, however, to allow the thermometer tube to pass: a bar, stretching across the inside of the boiler, at the same height nearly as the centre of the cork, supports the thermometer near the bulb, when the cork (thermometer and all) is inserted pretty tightly in its hole. As much of the tube of the thermometer is exposed as will show the division below the boiling-point; and the joint between the tube and the pipe, which projects a little, is made good with a binder of very thin vulcanised India rubber. Distilled water is poured into the boiler, but not so much as to touch the thermometer, which is thus boiled in steam. There are some round holes in the flat top which can be closed sufficiently by flat pieces of brass, and the mounting of the boiler admits of applying two or three spirit-lamps below, which are sufficient to bring it to a pretty smart boil.

When the steam rises strongly, the flat pieces on the top of

* Snow and ice must both be carefully drained, and both should be melting. When the snow or ice is wet the freezing-point stands too high.

† I have always applied to my colleague, Professor Miller, for the information, formulæ, &c., which I required; and have only had to select from the abundant store which he has forwarded me. I beg here to thank him, even more for the readiness and heartiness with which he has assisted me than for the assistance itself, valuable as it has been.

the boiler begin to chatter, and it is certain that the necessary heat, at least, is attained. By removing one, two, or three of the flat pieces, it will be found that in a little time the position of the mercury becomes steady, and is not affected by closing one of the holes or unclosing another. This steadiness of the boiling-point, whether the steam issues languidly or with considerable vehemence, is rather a puzzle to me, but the fact is quite certain.

A measure of the reading which corresponds to the end of the mercury (by telescope and micrometer as before), and a reading of a standard barometer (with its attached thermometer), the cistern of which is on a level with or at a known height above or below the boiler, give, by a formula, that reading of the boiling-point which corresponds to a pressure of 30 inches, or any other pressure that may be assumed. I propose, however, to determine the relation of the boiling-point at different pressures experimentally.

Two or three cautions must be observed. The boiling must continue a little time, as I think I have observed, to allow the bulb to arrive at its full expansion. The water must not be allowed to get too low, or the flame may heat the boiler *above* the water; and in that case the apparent boiling-point of the mercury will begin to rise.* Lastly,—the caution cannot be too often given,—the line of sight of the telescope must be perpendicular to the tube at the boiling-point, very nearly, or the plague of parallax, no small plague if the tube is thick, will come in.

In contriving my own apparatus I added an appendage, which is certainly satisfactory to a scrupulous observer, though I have scarcely derived any other advantage from it, viz. a steam-gauge in the other end of the boiler, which is thus out of the way of the thermometer and its cork: the gauge is inserted above the water-mark, and so contrived that the steam as it condenses flows back into the boiler. By placing rings to touch the two surfaces of the mercury in the gauge when the water is not boiling, or when all the holes are open, it is easy to satisfy yourself that there is no pressure when the boiling-point is noted, although the steam may issue with considerable violence.

If now, without loss of time, the thermometer thus boiled be placed in ice as before, and the freezing-point ascertained, it will be found to have fallen rather less than 0°·3 Fahrenheit.† The freezing-point rises, however, very rapidly—a very sensible quantity in 24 hours—but I cannot say how long a time it takes to resume its former stand. I am pretty sure that it does return exactly, in time; at least my observations hitherto agree to confirm this

* This happened to me once, and rather alarmed me, till I made out the cause. There are many ways—by balance, for instance— of ascertaining the quantity of water. I have applied a tube, bent upwards, which is inserted below the water-mark, through which I can also feed the boiler; but I have not yet used it.

† This, at least, has been constantly the case with me when the thermometers had been long enough made to have come to a steady stand before determining the freezing-point.

opinion; though I have heard from an eminent foreign maker that he does not find quite the same thing.

I will here venture to throw out a hypothesis, which I do not remember to have seen suggested elsewhere, and which I propose to examine as soon as I have time; viz. that the law of expansion is not so simple as has been generally supposed, but consists of two terms. One of these simply depends on the temperature of the body at the time; the other is a function of the highest temperature to which the body has been elevated, and the time since elapsed. There are several anomalies which I have seen, or heard of, which might, I think, be solved by this law, if it should turn out to be a true one. There is, too, some analogy with a well-known fact, that a piece of metal which has suffered violent constraint, such as hammering, rolling, &c., has a tendency to relieve itself, as it were, in time. I believe that some of the great variations which seem to have taken place in our standards of length may be referred to one or other of these causes; the lengths have been set out on bars too recently cast, or forged; or the metal, having been constrained by rolling or drawing, has struggled to release itself after the division.*

To return, however, to the thermometers. Having got contemporaneous freezing and boiling-points, and the errors of the divisions, I find the degree of Fahrenheit which corresponds to each tenth or fifth degree, and interpolate for the rest. After a considerable time I determine the freezing-point anew, which should agree with the *first* value, and add the proper correction to the table, if already formed, or use it in forming my table of equivalents of Fahrenheit.

If I am right in my belief, that the freezing-point of an old thermometer begins to fall even at so moderate a temperature as 100° Fahrenheit, it is clear that a *standard* thermometer, in the common acceptation of the words, *i.e.* a thermometer which will correctly indicate all temperatures from freezing to boiling, is not to be had, except the thermometer be boiled just before every observation,† when, alone, if I am right, the thermometer is correct at all temperatures below boiling.

For the ordinary usage of a standard thermometer, *i.e.* marking with great exactness all temperatures from a little below freezing to our greatest summer heats, I prefer using a well-seasoned thermometer divided into 120. The freezing-point is determined as before, and a point between 70° and 80°, or thereabouts, from a comparison with several *generating* thermometers, which have come to a steady freezing-point. By placing the thermometers in a box of water, and comparing with several generators, using the precau-

* The process of annealing might, perhaps, cure this restlessness in the particles of metal; but time would still be required, if this hypothesis be true, before the mass took up its final and permanent *stand*.

† This would be necessary in measuring the heat of hot springs, if 0°·1 or 0°·2 be sensible in such observations.

tions so often described, it is, I think, possible to get an upper point with sufficient accuracy.* A table, assigning the corresponding values of Fahrenheit, is to be constructed for each thermometer; and these, I should hope, would be truly *standards*, within the limits for which they are designed. A verification of the freezing-points, after a considerable time, would be all that could be wanted to entitle them to perfect confidence. In this way, by the use of two forms of thermometer, it seems to me that all temperatures up to boiling-water may be estimated. It is indeed possible that an extreme degree of cold may alter the freezing-point, but a mixture of salt and snow did not do so; though I own the experiment was carelessly made, not belonging particularly to the subject on which I was and am engaged.

I assume that the thermometer tube is horizontal. In a well-made instrument the mercury will not break at the neck or run to the other end, unless the tube be considerably inclined downwards, and my concern is only with the best tools and the best mode of using them. There is this objection to placing the thermometer upright, that the pressure on the interior of the bulb, when the column is long, enlarges the bulb very sensibly, and so causes the mercury to stand *lower* than it should do. In some thermometers with large thin bulbs, I think that the difference of reading between a horizontal and vertical position is at least half a division. But as in many cases a thermometer *must* stand upright, its errors, when upright, should be determined by comparison with a standard thermometer arranged horizontally. If this were carefully done, at a high and low temperature, the correction due to this cause for the upright thermometer might be applied to the tabular errors or table of equivalents. Perhaps, with care, a thermometer might be compared with itself, in the two positions; by continuing the observations alternately for some time, the effect of small changes of temperature would be eliminated.

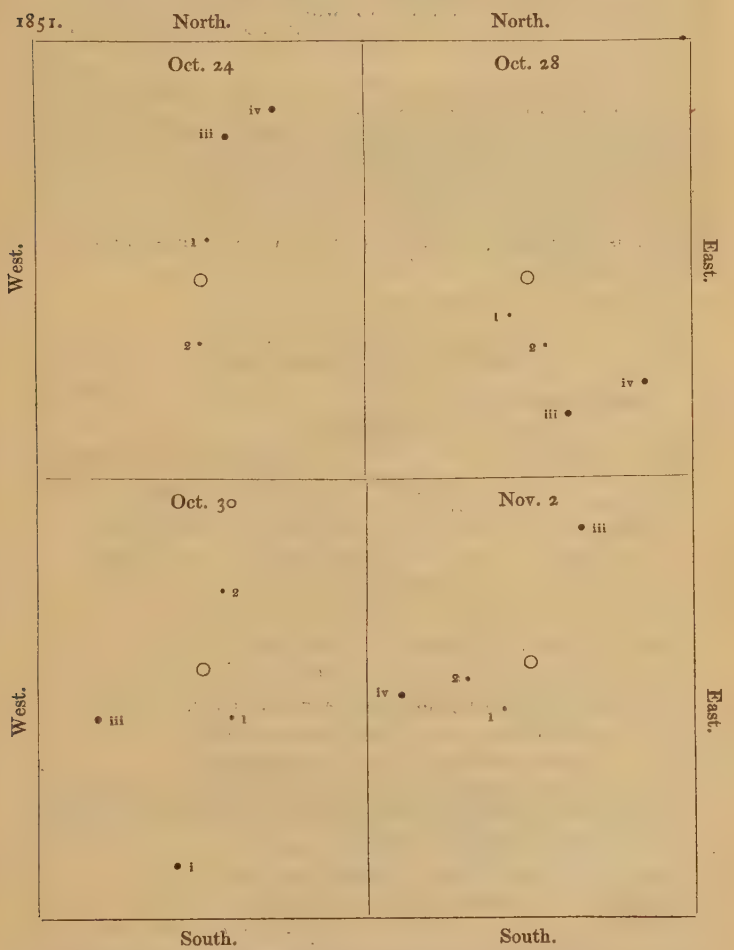
I conceive that all *nice* thermometrical observations should be made by telescope and micrometer; the telescope should move on a graduated bar, so as to prevent the possibility of parallax. If this refinement is thought needless, a reading lens may be slipped along a coarsely-graduated rod, when the reading will be free from all uncertainty except that of estimation.

It is obvious that, for most purposes, a thermometer which requires the use of a special table, of a specially-mounted telescope, and of a microscope, will not be required. I conceive, however, that a standard thermometer would always be useful as a check upon commoner instruments. To accompany the standard yard they are absolutely necessary, and I hope to fit out each yard with its ther-

* I compare with four, five, or six generators, and take a mean. The generators are perfectly independent, and any small errors in measuring the divisions or fundamental points probably nearly compensate each other. The water should be at the temperature of the room.

mometer. Whether it will be worth my while, or that of any other person, to make more than are wanted for this purpose, there is no need to consider at present. Perhaps one might be useful to a scientific-instrument maker, or in an observatory, for the purposes of regulation. I do not know whether in other physical researches, an accurate, but from its mass sluggish, thermometer is wanted, but I should think it probably would be useful in ascertaining the range and scale of more sensitive indicators.

Diagram of Uranus and his Satellites as seen by Mr. Lassell with his 24-inch Newtonian Reflector, equatoreally mounted.



The large dots iii and iv are the two bright satellites from which the positions of the new satellites were generally estimated or measured.

Extract of a Letter from Mr. Lassell, dated Nov. 3, 1851.

"I am now quite able to announce to you my discovery of two new satellites of the planet *Uranus*. I first saw them on October 24th with a strong persuasion that they were really attendants on the planet, and obtained further observations of them on the 28th and 30th of October and also last night. The observations are all perfectly well satisfied with a period of revolution of almost exactly four days for the outermost, No. 2, and 2·5 days for the closest, No. 1. They are therefore both considerably within the nearest of the two bright satellites, and even within Sir William Herschel's *first* satellite, to which he assigned a period of about 5 days 21 hours, but which satellite I have never yet been able to recognise.

"These new satellites are very faint objects, probably much less than half the brightness of the conspicuous ones, and generally the nearest has appeared the brightest. All four were steadily seen at one view in the 20-foot equatoreal with a magnifying power of 778 in the more tranquil moments of the atmosphere. The finest definition of the planet and freedom from all loose light in the field of view, is necessary for the scrutiny of these most minute and delicate objects."

ERRATA ET CORRIGENDA.

Omitted, page 1,

EGERIA.

South Villa. (MM. Bishop and Hind.)

1850.	Greenwich M.T. h m s	R.A. h m s	Dec. ° ′ ″
Nov. 12	9 52 5	1 51 21·87	+8 17 10·6
14	6 27 3	1 49 35·09	+8 21 28·3

Of about 9.10 mag.

Page 73, line 18, *for* Woolgar, *read* Woollgar.
— 64, — 12, — altitudes, — altitude.
— 63, — 3 from bottom, *for* 0·137, *read* 1·137.
— — last line, — 0·720, — 1·720.
R.A. of Weisse xxi. 937, is 1^m in defect.
Astron. Nachrichten, No. 760, Berlin observations of *Irene*, *for* May 14 and 16, *read* 24 and 26.
— 187, line 9 from bottom, *for* 5·4, *read* 5.4
— — 4 — — 5·6, — 5.6
— 188, — 1 from top, — 3·4, — 3.4
— — 4 — — 3·4, — 3.4
— — 11 — — 3·2, — 3.2
— 196, — 1 — — case, — ease.

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Académie des Sciences, Comptes rendus hebdomadaires, 4to., vol. xxix., 1849; xxx., xxxi., 1850; xxxii., 1851. *Paris, V.Y.* — L'Académie des Sciences.

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